

## Thoughts from the Tank

LORD ROTHSCHILD, head of the government's Central Planning Review Staff, delivered a serious warning to Britain in a speech to the Agricultural Research Council this week. Although the speech was billed as a preview to the Letcombe Laboratory's Open Day on "Root Function and the Soil" it is difficult to believe that Lord Rothschild was only addressing himself to those involved in scientific research. Nevertheless a train of thought now familiar to scientists dominated his speech. His main point was that unless corrective action is taken, Britain is going to slip steadily down the European economics league until by 1985, say, GNP per capita in Great Britain will be only half what it is in France and Germany. Such a slide could, in his view, only be counteracted by strong measures. We should abandon delusions of grandeur which were appropriate to Victorian times and "realise that we have neither the money nor the resources to do all those things we would like to do . . ." This was coupled fairly obviously in his speech with remarks on the difficulty during times of high unemployment of scrapping Concorde "even if that were a good idea"; or the difficulty of stopping thinking it reasonable to have four different types of nuclear reactor on the go at the same time. It was necessary that "every man and woman in the country be made aware of the dangers and difficulties ahead".

Thus Lord Rothschild added his voice to several influential ones heard recently on the subject of the decline of Britain as an economic power. The British, it is true, are prone to extraordinary fits of self-denigration, aided and abetted by the daily press, but warnings in the past year that we shall end up a nation of peasants and now this authoritative pronouncement have rather more to them than a quirk of national character.

One should say at the outset that Lord Rothschild has done a great public service by speaking out at last. There is bound to be a certain amount of tut-tutting in Whitehall over a civil servant (even if a temporary one) telling the public the conclusions of work for which the public has paid. Perhaps a healthy precedent is being set and more government experts will follow suit. The question of propriety is totally irrelevant, however, to the much broader issue of whether what has been said is true, whether it is desirable to remedy it and if so how.

It is generally agreed that Britain's growth rate is not that of other countries and it needs no more than the binomial theorem to demonstrate that we shall lag behind increasingly in the 1980s if present trends continue. And present trends can only be reversed by some dramatic action. A very serious cutback in the government's spending on symbolic items would make a lot of sense, and Concorde is a most logical target for this. There is a great danger that when Concorde has its certification and is demonstrated not to pollute as much as the environmentalists had feared (or hoped), the country will breathe a sigh of relief, mentally write off the hundreds of millions spent and declare that all's well that ends well. This is a problem that will arise repeatedly in the future—that public attention is so focused on peripheral objections

that once they have been overcome or shown groundless there is no concern left for the central issue of whether a project should have been done at all.

Lord Rothschild, however, calls for more than just fiscal caution from government in its big spending. He uses World War II analogies to urge a new spirit in the country. "In those days everyone got down to doing what the country needed him to do." Here he is on much more dangerous ground.

He is following the same path and he is likely to have as little success as did the Duke of Edinburgh with his celebrated and blunt advice some years ago. The failure of past threats of impending British decline should be sufficient warning to Lord Rothschild that there is something flowing deep in the national character which resents gratuitous advice from on high. The social divisiveness which still lingers in Britain impedes a consensus on improving productivity and any attempt to urge people to work harder is bound to create the cynicism which adds to the divisiveness. Will 5% more effort (even if possible) mean 5% more income?

Furthermore a divisive project such as Concorde which siphons off government money into faster trips for richer men can be seen as standing in the way of subsidies for more effective ground transportation which will improve the standard of living for many more people. This is undoubtedly what Lord Rothschild is all about but, bluntly, is he the best man to say these things? Our man-in-the-street wonders how his office has remained so quiet on the subject for three years suddenly to emerge threatening and advising.

Another question remains. Is an appeal (regardless of whence it comes) for help in economic growth rejected unconsciously by a large proportion of the public at present? This seems an issue of high importance as it may be a pointer to an increased reluctance in the years to come to work harder for purely economic purposes. To a certain extent, Lord Rothschild has anticipated this question by acknowledging that this is, by and large, a very nice country to live in, and thus a reasonable reaction is to wonder why there is so much fuss about economic indicators. His own reply is that lack of economic buoyancy leads to reduced capital expenditure on raising the quality of life, and there is undoubted truth in this. The problem, however, is that Britain may just be weary of industrial growth and may be saying in quite a sophisticated way that it wants out regardless of the cost. Several years ago Mr Harold Wilson was much taken with the idea that Britain should play Athens to the United States' Rome. Much has changed since then, of course, but the idea of a different type of development for different countries is one which deserves serious attention. Rather than attempting to whip up some fervour for economic growth in a country that may be on the verge of rejecting it as a national aim, Lord Rothschild might well put his and his unit's considerable abilities to work in pondering this rejection and seeing how we can live with it rather than fight it.



# Using Prediction Properly

UP TO 1970 no one had yet predicted an earthquake and there was no prospect of success. Certainly *Nature* received and declined a large number of manuscripts seeing strange coincidences in the time and location of past earthquakes and proposing, either with the aid of the Moon, the Sun or some strange and as yet undiscovered symmetry within the Earth, that there would be a quake in South America within the next five years, or some similar impossible-to-fail prophecy. As a scientific discipline, however, prediction did not exist.

The change has been remarkable. Not only have a whole class of premonitory symptoms been identified for certain earthquakes, but there are the makings of a physical theory. We need not elaborate here—last week we reviewed the most recent work (*Nature*, 245, 121). It seems that the rocks in the vicinity of a fault prepare themselves for an earthquake in the same way that one takes a deep breath before sneezing.

This is good news, especially as many geophysicists, with much justification, could see no clear-cut reason why there had to be any premonitory symptoms at all, let alone ones that could be easily detected. It is necessary to say, of course, that we have not moved from a state of complete ignorance to one of complete understanding—there are as yet few case histories and many doubts; nonetheless the mood is clearly optimistic.

It is now urgently necessary to think seriously about the social issues that this new knowledge raises. The situation is in some ways similar to that in 1939 when nuclear fission suddenly became a reality. The prospects for society are neither uniformly good nor uniformly bad and there is still time, but relatively little, to explore ways in which good prospects can be encouraged and bad ones minimised. What are the prospects ten years hence?

A rather clear warning of many earthquakes will be available. Their size, their location to within a few tens of kilometres and their time of occurrence to within a month or two, should be predictable.

Techniques used will be relatively cheap and involve no complex new equipment that only governments, for instance, would be able to afford.

Interpretation will not require unusual skills—no more, probably, than a skilled technician would have.

It will be impossible to suppress such information or keep it behind some security screen.

The technology and skills will be marketable and exportable rather like an air traffic control system.

Property values will be affected by earthquake warnings.

What little is known about population movements suggests that there will be a marked reluctance to leave homes for months on the basis of scientific advice.

It will still be next to impossible, given even rather precise predictive information, to say with any confidence what damage an earthquake will cause.

Earthquake prediction is founded on the same physical basis as the fairly successful but as yet very modest scale work on earthquake control. Thus success in prediction will lead to demands for success in control.

All of which suggests that without sustained wise and firm action the benefits will be extensively mixed with

misadventures. Will earthquake prediction and control need a Hiroshima of its own before there is widespread thought on the subject?

It is not easy to see the most logical steps to be taken at present to keep this new skill under strict control. While there is an obvious need for research to continue with as much vigour as possible, it would be folly to leave to the scientist the job of public information. And yet the scientist may be infuriated if his clear conclusions are watered down in the interest of public tranquillity. From a previous era, when the only prediction could be that if it has happened in a spot once it can happen again, there are many examples from all over the world of rebuilding for convenience exactly where the previous damage had occurred. More must be expected of an earthquake defence policy in the future.

The problem is that there is no organisation at present remotely suited to the job of preparing such a policy. The skills that will be needed are as much political, military and financial as scientific, indeed the sort of organisation that may be best fitted to assess and make decisions could well have many features much closer to those of a defence ministry than those of a science ministry.

Can such an organisation be international, for instance under UN auspices? It is not clear whether within UN confines the operation could transcend serious political and administrative obstacles. A more realistic analogue is the international corporation with vigorous activity within each of its members directed at the specific needs of the country and drawing its financial support from that country and yet transferring management and skills across frontiers in response to immediate needs.

The organisational character is disputable, the need for a rapid response to a new-found skill is not. Those who doubt this should spend ten minutes listening to Dukas' *L'Apprenti Sorcier*.

## 100 Years Ago



The Frog is the never-failing resource for the physiological experimenter. It would be long indeed to tell the sufferings of much-enduring frogs in the cause of Science! What Frogs can do without their heads? What their legs can do without their bodies? What their arms can do without either head or trunk? What is the effect of the removal of their brains? How they can manage without their eyes and without their ears? What effects result from all kinds of local irritations, from chokings, from poisonings, from mutilations the most varied? These are the questions again and again addressed to the little animal which perhaps more than any other deserves the title of "the Martyr of Science."

From *Nature*, 8, 470, October 2, 1873.



## OLD WORLD

# Science and the Cambridge By-pass

LAST week's announcement by Mr John Peyton, Minister for Transport Industries, that approval has been given for a northern by-pass for Cambridge and for the extension of the M11 to form a western by-pass has raised fewer hackles in Cambridge than might have been expected.

The new roads will affect the 5 kilometre radio telescope at Lord's Bridge, the University Farm, the Medical Research Council's Animal Behaviour Unit, the National Institute for Agricultural Botany and the Agricultural Research Council's Plant Breeding Institute.

But the inspector's 300 page report, which has now been published, following an inquiry that took from February to July last year, seems to have mollified many fears, or at least set them in abeyance.

More than 170 objections to the western by-pass were entertained by the inspector, and the complaints of the research stations were among the weightiest.

Professor Sir Martin Ryle, Astronomer Royal and Director of the Mullard Radio Astronomy Observatory, raised objections to the radio interference that is likely from the new road. The inspector, Major General R. C. A. Edge, states that the objection "merits most serious consideration", but finally concludes that "during the forecast 20 years useful life of the 5 kilometre telescope, there will be no serious threat of interference with the operations of the Lord's Bridge Radio Observatory" providing certain conditions are met.

Sir Martin's fears were that in the short term engine ignitions and the use of v.h.f. and u.h.f. radios by police and service vehicles could interfere with the telescope, and in the longer term, that motorway guidance systems might be introduced during the next twenty-five years. These would be microwave devices which might radiate in the frequencies received by the telescope, or produce undesirable spurious harmonics.

The first of these fears is calmed by the planned provision of a bank and a metal screen some 5 metres high. But the second is not so easily dismissed. The inspector's report quotes evidence from the Department of the Environment that some sort of automated guidance "is likely to find a successful application in this country before 1980". Fog is common in the area through which the motorway will pass, and Sir

Martin pointed out in his evidence that if a system that does successfully prevent accidents is developed, there will be enormous pressure for its introduction. "I must therefore regard it", the inspector concludes, "as a serious possibility that the system will be installed on the western by-pass some years before the end of the 5 kilometre telescope's working life".

Sir Martin has, however, received some assurances from the Ministry of Posts and Telecommunications and the Department of the Environment that the telescope will be protected from interference, and that there are no plans to allocate the bands used for radio astronomy to new radio-location devices.

Sir Martin said this week that his fear is that when a new system is introduced it will have to be mass produced to be usable on all cars. It is therefore likely that the transmitters will produce spurious harmonics which could affect the telescope. The inspector states that he is aware of this problem, which Sir Martin aired at length at the inquiry. The Department of the Environment is also aware of it, and stated in evidence that "(microwave) equipment would not be approved if it seriously interfered or was capable of interfering with other forms of equipment including radio telescopes".

Given this assurance, the inspector concludes that the road can go ahead,

## MARINE RESEARCH

### Old Boats for New?

THERE are fears that the work of the Marine Biological Association at Plymouth is likely to suffer in the near future for lack of an ocean-going research vessel.

Plans to provide the association with a new research vessel to replace the twenty-year-old *Sarsia* have been under consideration since 1968, but the present shortage of money and the policy of the Natural Environment Research Council (NERC) of centralising ship facilities has meant that plans to build a new 130 foot ship at a cost of between £400,000 and £600,000 have been repeatedly deferred. Originally the new ship was to enter service in 1973.

Since the beginning of the century, when the Marine Biological Association (now financed through NERC) was already responsible for fisheries research, the association has had a deep-water vessel—first the *Huxley*, then the 90 foot steam drifter *Salpa*, followed in 1953 by the *Sarsia*.

At present the association's fleet consists of the 128 foot *Sarsia*, which has just returned from an expedition to the Sognefjord in Norway, with three smaller boats of 35, 45 and 60 foot.

The *Sarsia*, whose research has taken it to the Bay of Biscay, to the southern North Sea and off the west coast of Ireland, has chiefly been used for studies of physical and chemical oceanography, the plankton and benthos of the western English Channel and approaches, and the physiology of shallow water fishes and cephalopods. Other studies have included subjects such as vision and

buoyancy of deep-sea fishes, the fauna of the continental slope between Ireland and Spain, the geological structure of the edge of the continental platform to the south-west of Britain, and the sediments of the eastern English Channel and southern North Sea. Much of this work could not be carried out with the association's smaller vessels, which cannot stay at sea for more than a day or so.

Work undertaken on the *Sarsia* has produced more than 240 papers during the ship's twenty years of life, and, although now obsolete when compared with other research vessels of her size, she may be used for a few years yet.

A final decision has not been taken on whether a new ship should be provided for the Marine Biological Association, but the indications are that if a new ship is approved it will be for the NERC's research vessel base at Barry Docks in Glamorgan. This possibility is causing concern to some oceanographers in the south-west, who fear that having to share a research ship based in Wales rather than at the western end of the English Channel will hamper work that is now easily carried out from the Plymouth base.

Only about a quarter of the association's research involves the largest of its vessels, but with France rapidly building up new oceanographic laboratories and a new university at Brest, in addition to the reorganised fishery laboratory based at Nantes, it may be that Brittany will have to continue the marine biological research in the western Channel and northern Bay of Biscay, areas which are at present patrolled by the ageing *Sarsia*.

but warns that "failure of the department in this respect would, in my opinion, amount to culpable irresponsibility".

The problems of the other institutes are somewhat different. As with the observatory, the Animal Behaviour Unit has not yet had a copy of the report to examine, but its initial reaction is one of relief that the inspector has recommended screening barriers to cut down noise from the motorway, which the unit claimed could seriously hamper work on animal calls. The inspector also recommends that the barrier "should be in position before construction work starts".

The University Farm suffers from the motorway in that a number of its experimental plots are involved. The farm serves as a research station for the Department of Applied Biology, and drainage and crop rotation experiments will be affected.

The crop experiments involved are all long term studies running for many years. One will be seriously truncated, one of the three portions of the experiment being completely obliterated with a second losing a small corner. A second experiment is in quadruplicate and one of the four blocks will be affected, but the inspector concludes "that loss is too small to render the results valueless". Professor James Beament, Head of the Department of Applied Biology agrees with this so far as it goes, but points out that having a motorway running along the side of the plot will change the atmospheric conditions. The drainage experiment will also be affected as the motorway runs along the lines of the drains. The Department of the Environment has undertaken to provide drainage to match that already in existence, and providing this is done—and it is not that simple a task—the effect of the by-pass should be minimal.

At the National Institute for Agricultural Botany, ten acres of land will be lost to the northern by-pass, but Dr Peter Wellington, the institute's director, says that there was so much advance warning that the road might be built that the institute has bought some land well away from the by-pass, and with the Department of the Environment's cooperation the effects should be minimal. The Plant Breeding Institute has greater problems, as the western by-pass cuts through its land, isolating five fields at the Harston end of the institute.

#### GLAXO AWARDS

### Maddoxian Success

THE eighth annual Glaxo Award for science writing in British national journals and newspapers has been made to Mr John Maddox, former editor of *Nature* and now chairman of *Maddox Editorial*. This year, three other awards



Mr John Maddox

have been made, each, like the national award, worth £500: for radio and television work, to Mr Alec Nisbett (BBC); for a journalist working in the regions, to Mr Geoffrey Lean (*Yorkshire Post*); and for work published in a technical or house magazine, to Mr Colin Tudge (*New Scientist*).

The awards are in the form of travelling fellowships; in making the presentation on behalf of Glaxo Mr J. G. N. Drewitt, one of the directors, cited in particular the breadth of Mr Maddox's writing, on topics ranging from the environmental issue to special relativity, during the calendar year 1972. Mr Maddox submitted the following for consideration: his book *The Doomsday Syndrome*, an article in *Daedalus* (Fall 1972), an article in *Nature* (236, 267) and *Nature* leaders (235, 63; 236, 417; and 239, 242).

#### INDIAN ASTRONOMY

### Forming a Central Voice

AT their fourth attempt, Indian astronomers have founded a national astronomical society. The Astronomy Society of India, as it has dubbed itself, came into being late last year and has now acquired 150 members throughout India.

The society, a private one financed entirely from its members subscriptions, is to hold its first general and scientific meeting at the Centre of Advanced Study in Astronomy at Osmania University, Hyderabad in January next year. The teaching of astronomy in India will also be discussed.

The new society is the result of the enthusiasm of Professor K. D. Abhyankar, now the society's secretary, who in 1971 sent a questionnaire to Indian astronomers proposing the formation of ASI. The response was enthusiastic. Indian astronomers have maintained for some years that a central forum was needed, but previous attempts to form an astronomical society have foundered.

The most recent was the Indian Astronomical Association formed in 1958, but this failed to achieve wide support.

The new society has already produced its first bulletin and has plans to publish a scientific journal, but according to Professor Abhyankar "considering the financial aspect we feel that it could be some time before this goal materializes". The society is attempting to recruit institutes as members to boost finances, and various government departments and agencies are being asked for support.

Having no official status, the ASI will not directly advise government, but "the society is bound to discuss in its meetings astronomical matters of national interest, and resolutions, if any, passed by the society will be forwarded to the government", according to Professor Abhyankar. The society also plans to examine the development of science in India, but "it is too early to say in which direction pressure may be applied".

#### ASTRONOMY

### Educating the British

THE Junior Astronomical Society—"junior" in terms of astronomical experience, rather than age—has just celebrated its twentieth anniversary with a meeting at the Royal Institution last week. Amidst the appropriate celebrations, which included a talk by Professor Dennis Sciama on the Origin of the Universe, a film about Apollo 17 and suitably festive refreshments, Professor Jack Meadows was allotted a few minutes to discuss astronomy education.

Fresh from the first meeting of the Royal Astronomical Society's Education Committee, Meadows was well placed to focus the attention of the JAS on this topic; over the next twenty years members of the JAS can certainly expect to play a part in relevant educational developments, even if the Society itself is unlikely to be formally involved with any moves by the Department of Education and Science.

Meadows sees the problem of astronomy education in Britain as one of breaking into the circle whereby little or no astronomy is taught in schools because there are few qualified teachers, and there are few qualified teachers because the subject is not taught. At university level, astronomy is reasonably well established as a vocational course; but there is a growing body of opinion that astronomy can provide a valuable ancillary course throughout the educational system, providing an insight into how physics and mathematics work in reality. One hopeful approach to breaking the vicious circle mentioned by Meadows lies in providing astronomy courses as part of in-service "refresher" training of teachers—but the DES will not at present finance such courses, he says.



## NEW WORLD

## Congress's Stand the Unkindest Cut Yet

by our Washington Correspondent

ALTHOUGH there have been many loud complaints on Capitol Hill in the past few months about the Nixon Administration's parsimonious attitude towards science, Congress has succeeded in giving the budget of the National Science Foundation perhaps the worst mauling it has received for years. Not only are the Senate and the House of Representatives both about to chop \$13 million from the Administration's already meagre budget request for the foundation (see box), but they have also managed to arrange matters so that basic research will be particularly hard hit. One NSF official estimated last week, for example, that more than \$30 million may have to be cut from the amount that the foundation was hoping to spend this year on research support.

The foundation's troubles can be put down partly to the tortuous nature of the Congressional appropriations process, but the chief problem is that the NSF has got caught in the middle of the bitter struggle between Congress and the Administration over which branch of the government should control the pursestrings. What happened, in short, is that while Congress has cut the NSF's total budget request, it has also decreed that some of the foundation's activities should get more money than the Administration wants to spend; in the process, basic research has been shortchanged. Moreover, if the Office of Management and Budget impounds any of the NSF's money—as it has in the past four years—the foundation will be in dire financial straits.

The National Science Foundation is one of the few government agencies for which Congress passes two separate budget bills—an authorisations bill and an appropriations bill. The former simply sets upper limits on the amount of money that can be spent on individual NSF programmes, while the latter stipulates exactly how much money should be made available to the foundation. The idea is that the authorisations bill sets priorities for the foundation's programmes, establishes guidelines for the committees which handle the appropriations bill, and provides an extra measure of Congressional control over the foundation's activities. But this year, instead of simply setting upper spending limits, the authorisations bill also establishes minimum spending levels for a few selected pro-

grammes, and it is those which are causing much of the trouble.

The reason for putting the minimum spending levels into the authorisations bill is to try to prevent the Administration from ignoring the priorities that Congress wants to establish for the foundation. In the past few years, Congress has been concerned about the level of funding for the NSF's education and graduate student support programmes, and it has consistently increased the budgets for those activities. But the Administration has simply refused to spend the extra money.

Specifically, the authorisations bill sets minimum spending levels for the foundation's science education, institutional improvement, graduate student support, and oceanographic vessel construction programmes. Together, those spending levels add up to some \$18.3 million more than the Administration wants to spend. It would have been fine if the appropriations committees had increased the Administration's total budget request for the foundation by at

least that amount, but they didn't. Instead, they decreased it by \$13 million.

The upshot is that if the Administration obeys its Congressional mandate, the budgets for those programmes unprotected by the minimum spending levels must be cut by a total of \$31.3 million—the \$13 million overall reduction in the foundation's budget, plus \$18.3 million which will have to be transferred to comply with the minimum spending requirements. The bulk of the unprotected programmes happen to be scientific research project support, national and special research programmes and support for national research centres—the foundation's basic research programmes.

But the problems do not end there, for the foundation's programme of Research Applied to National Needs (RANN) has been caught in exactly the same predicament. The authorisations bill stipulates that "no less than" \$25 million must be spent on RANN projects in energy research, and \$8 million on earthquake research. The Adminis-

## BUDGETS

## Hit by a Limousine

by our Washington Correspondent

CONGRESS has finally agreed on the size of the budgets for the National Science Foundation and the National Aeronautics and Space Administration for the 1974 fiscal year. Both agencies are set to receive less money than the Administration had requested. But final passage of the appropriations bill which contains their budgets is being held up by a strange dispute over the use of chauffeur-driven limousines by government executives. Earlier this month, the bill was sent back to a House-Senate conference committee to iron out disagreements on the matter.

The same conference committee managed to agree in August on how some \$19,000 million should be spent by the Department of Housing and Urban Development, NASA, NSF and some other government agencies. But it failed to reach agreement on a provision, sponsored by Senator Proxmire and passed by the Senate, which seeks to prevent government executives in agencies covered by the bill from riding to work in government limousines.

Both the House and the Senate have now agreed to the figures contained in

the appropriations bill, and they will not be reopened to debate, but the Senate continues to insist that the limousine restrictions be included in the bill while the House is equally adamant that they be kept out. Until one side or the other backs down in the conference committee, the appropriations bill will hang in limbo. The General Accounting Office has, however, been requested to make a full study of the use of limousines throughout the government, and the Senate representatives in the committee are expected to back down until the study has been completed.

As for NASA and the National Science Foundation, the appropriations bill contains \$3,002 million for the former and \$596.6 million for the latter, which is some \$13.9 million and \$13 million respectively less than the administration had requested. In the past few years, however, the Office of Management and Budget has impounded some \$59.8 million in funds appropriated by Congress for NSF, and if that money is released this year—as the Administration's budget indicates—the foundation will have some \$628.5 million available. That would give the foundation \$13.5 million more than OMB released last year, but it still falls well short of the \$645.7 million that Congress appropriated last year.

tration had requested \$20.7 million and \$7 million respectively for those programmes, thus Congress has decreed that an extra \$4.3 million must be found for them. But, at the same time, the final version of the appropriations bill has reduced the total request for RANN from \$80 million to \$72 million, which is only \$2 million more than it received last year. Again, the upshot is that RANN programmes unprotected by minimum spending levels must suffer a reduction of \$12.3 million.

The authorisations bill has already been completely agreed to by Congress and signed into law, thus it cannot be altered. As for the appropriations bill, it is highly unlikely that it can be altered at this stage. Although the bill has not yet finally cleared Congress, the figures for the NSF have been agreed to by both the Senate and the House and they will not be reopened to debate.

One remedy, of course, would be for the Administration to do exactly what it has in the past—ignore its Congressional mandate by impounding the extra funds voted for the education programmes and spread the \$13 million reduction as it sees fit. But the Administration is already facing some 50 court challenges of its power to impound funds which have been appropriated by Congress in the normal way, and it would be skating on very thin legal ice if it attempts to withhold money to which Congress has attached a minimum spending requirement.

#### SOVIET UNION

## Support for Dissent

by our Washington Correspondent  
SOVIET physicist Andrei D. Sakharov and other dissident intellectuals in the USSR received their first official support from the United States Congress last week. A resolution, roundly condemning the "campaign of the Soviet government to intimidate those who have spoken out against repression of political and intellectual dissent", was attached to the State Department Appropriations Bill and passed by the Senate without opposition. Although it expresses only the Senate's concern about the matter and is not binding on the Administration, the resolution also calls on President Nixon to "use the medium of current negotiations with the Soviet Union, as well as informal contacts with Soviet officials, in an effort to secure an end to repression of dissent".

Sponsored by Senator Walter F. Mondale, a liberal Democrat from Minnesota, the resolution provides fair indication of the outlook for other, more hard hitting, measures designed to force internal change within the Soviet Union that are looming on the legislative horizon in the United States. Those measures, if enacted, could cast a cloud

over the growing detente between the United States and the USSR, a significant part of which is the wide range of scientific and technical agreements recently negotiated.

The most prominent of such measures is the so-called Jackson Amendment to the Trade Bill, which is an attempt to use the Soviet government's desire for expanded trade with the United States to force it to liberalise its attitude toward dissenters and to allow Jews and others the freedom to emigrate. The amendment, sponsored by Senator Henry M. Jackson in the Senate, and in the House of Representatives by Wilbur D. Mills, the chairman of the House Ways and Means Committee, and by Charles Vanik, would prevent the administration from relaxing tariff barriers and extending credit to the Soviet Union until the Soviet government grants its citizens freedom to emigrate.

The outlook for the amendment in the House is difficult to predict, but it seems clear that some such restrictive measure will be attached to the Trade Bill. The Jackson Amendment has already gathered 77 co-sponsors in the Senate, and with the lack of opposition to Mondale's resolution, it would be surprising if it did not pass easily. Attention at present is focussed on the House Ways and Means Committee, which has the bill under consideration—it is a legislative quirk that all trade bills must be passed by the House before they can be taken up by the Senate—and the committee is under pressure from an odd assortment of bed-fellows.

Arguing in favour of the Jackson Amendment is a coalition of liberals, conservatives and Jews, while the Administration and large corporations are taking the line that the United States has no right to interfere in the internal politics of the countries with which it trades. But one of the most prominent supporters of the Jackson Amendment is Sakharov himself. He wrote an open letter to the United States Congress on September 14, which has subsequently appeared as a full-page advertisement in several prominent United States newspapers, in which he argued that the amendment "is made even more significant by the fact that the World is only just entering on a new course of detente and it is therefore essential that the proper direction be followed at the outset. This is a fundamental issue, extending far beyond the question of emigration". Sakharov's letter ended with a plea that the United States Congress "will realize its historical responsibility before mankind and will find the strength to rise above temporary partisan considerations of commercialism and prestige".

Three chief scenarios are being discussed. The first is that the Jackson

Amendment will be passed by both the House and the Senate. The second is passage of a less restrictive amendment, which would allow liberalization of trading regulations with the Soviet Union if there is reasonable progress in the USSR toward free emigration. And the third is that the provisions relating to tariff barriers with the USSR will be dropped entirely from the House version of the Trade Bill, that the Jackson Amendment will be passed by the Senate, and a compromise will be worked out in a conference committee. At present, however, it seems that straight passage of the amendment is the most likely alternative.

What effect is all this having on scientific relations between the two countries? Earlier this month, the National Academy of Sciences sent a warning to Soviet officials that further harassment of Sakharov will jeopardize the scientific agreements between the two countries, and last week two other scientific organisations issued statements on the matter. The first, drawn up by the Committee of Concerned Scientists, signed by 23 prominent United States scientists and delivered to Dr Keldysh, President of the USSR Academy of Sciences by Dr Edward Stern of Washington University, warns that "it will become increasingly more difficult to continue scientific and technological cooperation with the Soviet Union as long as the Soviet authorities persist in their present repressive policies".

The other statement was put out by the Federation of American Scientists, an intellectually powerful lobbying organisation which has persistently argued for arms control. The FAS statement calls on United States scientists to work for liberalisation of the internal policies of the USSR, arguing that the present military accord between the United States and the USSR is only a "temporary and fragile" solution to the arms race, which is too easily reversible in the absence of internal criticism.

But as far as implementation of the joint scientific and technical agreements is concerned, the State Department's office of international scientific affairs has so far received no letters or telegrams protesting about the agreements, and has not yet encountered any lack of enthusiasm for implementing them. Moreover, there seems to be no reaction—either direct or indirect—from Soviet officials to the NAS protest, and arrangements are proceeding without hindrance for the second meeting of the international Joint Commission which is set to meet in Moscow in November. But one State Department official acknowledged last week that the possible effect of the Jackson amendment on the scientific and technological accords is difficult to predict.



## NEWS AND VIEWS

## NASAs Sensors Focusing on Comet Kohoutek

THE imminent arrival of comet Kohoutek in the inner regions of the Solar System is causing a flurry of activity not just among ground-based observers but also among those responsible for planning and coordinating the activities of scientific satellites and space probes. In the long run, it might well turn out that Kohoutek's most important achievement in this respect has been to raise interest in comets to the point where the National Aeronautics and Space Administration is considering more seriously than ever before the possibility of a comet interception mission, perhaps to comet Encke or Halley's Comet, some time in the immediate future. The long lead time required for such a rendezvous or flyby mission makes it impossible to achieve a fruitful interception with Kohoutek, but the data which are likely to be gathered from this comet over the next few months will provide the background information, as well as the enthusiasm, necessary for the success of such missions in the future.

So what are the puzzles about comets which observations of Kohoutek might resolve, and how is NASA, in particular, setting about their resolution? According to the most generally accepted theory, the most important part of a comet—and the least well understood—is the nucleus. Although only a few miles across, the nucleus produces all of the material seen in the spectacular tail, as it is heated by the Sun. Most astronomers prefer the "dirty iceberg" model of the nucleus, in which a rocky core is both permeated by various ices and surrounded by an icy mantle which contains small dust grains. The water vapour and other gases which sublime off as the comet nears the Sun are soon ionized, and carry with them dust particles. So one particular feature of comet Kohoutek is the possible development of an  $H_2O$  atmosphere; this possibility, and ways to detect such an atmosphere, are discussed by Ip and Mendis on page 197 of this issue of *Nature*. Other molecules, such as carbon dioxide, methane and ammonia, may also be present in comets, and evidence of their presence in Kohoutek would support this model. In spite of the almost overwhelming popularity of the icy nucleus model, however, there is an alternative possibility. This is that a comet is just a swarm of dust particles, and does not contain a central nucleus at all. One of the strongest supporters of this idea is Lyttleton, who has previously pointed out that even if the idea of a solid nucleus is correct "no such finite object could ever actually be seen short of sending a probe to the comet" (*Mysteries of the Solar System*, Clarendon Press, Oxford, 1968) but who considers on page 200 of this issue of *Nature* observable features of comet Kohoutek which, if they are indeed observed, would provide strong support for the dust cloud model.

The observations which will help in assessing the merits of these models and other ideas will come from a multitude of spacecraft, and as well as the usual ground-based observations a special observatory for studying comets is now being completed in New Mexico, 10,600 feet up on South Baldy mountain. But the observing platform which is undoubtedly destined to receive most publicity is Skylab. According to a report in a recent issue of *Aviation Week & Space Technology* (September 17, 99, 102; 1973),

scientists planning the most fruitful use of Skylab for observations of Kohoutek have been "pleasantly surprised" by the cooperation and flexibility of mission planners; this marks a profound change from earlier manned missions, where it was felt that science took very much of a back seat.

Even the launch date of the last Skylab mission has been revised to allow observations of the comet at the most suitable time, in the immediate post-perihelion period (December 28, 1973, onwards). Launch on November 9 with a 56-day mission would end observations rather early, from the astronomers' point of view, but launch may be delayed to November 21, or the mission extended, to allow a longer post-perihelion observing time. In addition to the impressive array of cameras and other sensors Skylab already carries, the next crew will also carry with them the same ultraviolet camera used on the surface of the Moon by the Apollo 16 team. With this camera, they will be able to photograph neutral hydrogen surrounding the comet, thanks to its Lyman-alpha emission. Possibly this might involve working outside the space station, because the camera has a 20 arc degree field of view and the mirror system by which onboard instruments are focused through the scientific airlock has only a 7 arc degree field of view. Ultraviolet observations of other comets by OAO-2 and OAO-5 produced surprising evidence that this hydrogen cloud can have many times the diameter of the Sun, making it a prime target for study.

Other Skylab experiments designated for studies of Kohoutek include a white light coronagraph; a far ultraviolet spectroheliograph and spectrograph; the scanning ultraviolet spectroheliometer; and instruments intended originally for ultraviolet observations of stars and airglow phenomena. Apart from information about the comet itself, the observations will also indicate the speed and direction of the solar wind near the Sun.

Other spacecraft being coordinated into one unified programme for studying Kohoutek are:

- Mariner 10, a Venus/Mercury mission due for launch on November 3.
- Pioneer 8, now in orbit around the Sun, which will lie beyond Kohoutek, as seen from Earth, between January 1 and 10, so that studies of radio signals from the spacecraft can be used to analyse the comet's tail.
- Copernicus, now in orbit around the Earth.
- OSO-7, which can make ultraviolet observations between December 27 and 29.

In addition, NASA will use a Learjet which carries an infrared telescope for observations before perihelion, and a larger airborne Infrared Observatory in a Lockheed C-141 in January, when the comet is higher in the sky, because the latter can only observe at 35° or more above the horizon. And in spite of the problems of optical detection of a nucleus, the question posed by Lyttleton might be resolved if a team at the Jet Propulsion Laboratory succeed in bouncing a radar signal of Kohoutek from the Goldstone tracking station.

J.G.



# Regulation of Vitamin D Metabolism

VITAMIN D plays a vital part in calcium transport: it mediates intestinal calcium absorption, bone calcium mobilisation and, probably, normal muscle activity. By these actions it prevents the lowering of blood calcium, deficient calcification in bone matrix and in cartilage, and muscle weakness (myopathy) which together characterise its specific deficiency disease, rickets or osteomalacia<sup>1,2</sup>. Vitamin D<sub>3</sub> (cholecalciferol) reaches the body in two totally dissimilar ways; first, in diets containing oily fish, eggs, animal liver and dairy products and, second, by formation in the skin (possibly with other active compounds) from the action of ultraviolet light on the physiological precursor 7-dehydrocholesterol (see figure). The relative importance of these two sources in human nutrition will be considered.

The dissimilarity between the two alternative sources of vitamin D produced much confusion in the first two and a half decades of this century. From then on further difficulties in interpreting human disease have continuously arisen with the discovery of bone diseases which closely resemble classical rickets and osteomalacia but which may be resistant to treatment with vitamin D in doses up to one thousand times the amount required to heal the simple nutritional disease. These diseases may be associated with disorders of intestines, liver or kidney, and also include a host of inborn and acquired errors of metabolism. More than thirty such diseases have been listed<sup>3,4</sup>. While their discovery and description continue even now, their understanding has been greatly simplified by recent advances in the understanding of vitamin D metabolism, advances which have indicated the importance of different, seemingly unrelated organs in controlling the ultimate expression of vitamin D activity.

In earlier studies there were speculations on the way in which intestinal calcium absorption is controlled. Nicolaysen<sup>5</sup> postulated the existence of an "endogenous factor" which in some way senses a need for and mediates the increased calcium absorption which he found in animals when they were maintained on low calcium diets. It was not, however, until 1966 that synthesis of radioactively labelled cholecalciferol of high enough specific activity<sup>6,7</sup> allowed later work to be done on the intermediary metabolism of vitamin D and thereby to clarify the probable nature of this "endogenous factor".

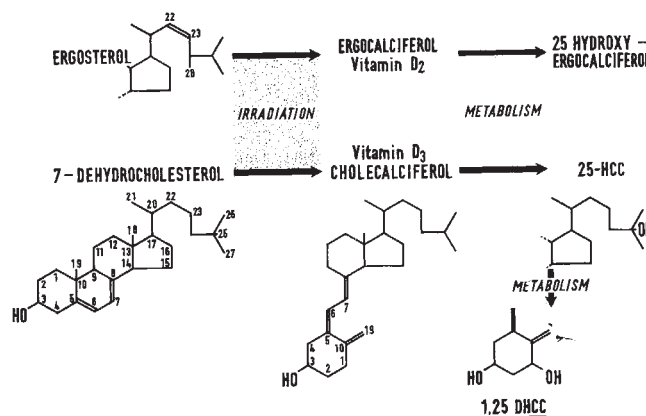
Cholecalciferol can now be thought of as a hormone precursor, and two further stages are required in its metabolism before it reaches its active hormonal form. In the first stage it is converted to 25-hydroxycholecalciferol (25-OH-D<sub>3</sub>) (ref. 8) and this compound is now recognised as the principal circulating form of the vitamin (see figure). Initial studies suggested that 25-OH-D<sub>3</sub> itself was the biologically active form; it seemed to be synthesised only by the liver, and synthesis seemed closely regulated by a feedback control mechanism<sup>8</sup>. Recent work has suggested, however, that some 25-hydroxylation of cholecalciferol may also occur in homogenates of chick kidney and intestine, and that the process is not strongly inhibited in the presence of excess product<sup>9</sup>. The possible physiological importance of other organs in the 25-hydroxylation of cholecalciferol, and the enzymatic kinetics of the reaction, may therefore require further study.

Vitamin D<sub>2</sub> (ergocalciferol), the well-known synthetic form of vitamin D, seems to be metabolised to 25-hydroxy-ergocalciferol (25-OH-D<sub>2</sub>) in identical fashion (see figure). Because both parent compounds have long been known to possess similar anti-ricketic activity in man it may be assumed that in man both 25-hydroxylated derivatives also possess equal potency. The 25-hydroxy derivatives of both forms (together designated 25-OH-D) can be measured by radio-stereo-assay<sup>10</sup>. High concentrations of circulating 25-OH-D are found in subjects with high occupational exposure to sunlight<sup>10</sup>. Concentrations of 25-OH-D increase rapidly during ultraviolet irradiation of the whole body, in spite of continuing vitamin D deficiency in the diet and in spite of the possession of a dark skin which has popularly been supposed to lessen the transmission of ultraviolet light<sup>11</sup>. 25-OH-D<sub>2</sub> and 25-OH-D<sub>3</sub> have been recently separated in human plasma; in the United States, where widespread fortification of foods with vitamin D<sub>2</sub> is practised, plasma 25-OH-D was found to be still composed largely of 25-OH-D<sub>3</sub> (ref. 12). These findings indicate that the effect of sunlight on the skin, rather than the composition of the diet, provides the chief source of vitamin D in most human communities. Evidence is also forthcoming of marked seasonal variation in the plasma levels of 25-OH-D in normal subjects (T. C. B. Stamp and J. M. Round, unpublished data).

After administration of 25-OH-D<sub>3</sub> a time lag of several hours is still evident before intestinal calcium absorption is stimulated<sup>13</sup>, and this finding suggested that further metabolism of 25-OH-D<sub>3</sub> to a more active form might be required. A more polar metabolite of 25-OH-D<sub>3</sub> was first isolated from chick intestinal nuclei in 1969<sup>14</sup>. It was found to have a more immediate and more powerful action than 25-OH-D<sub>3</sub> (ref. 15) and to be produced from 25-OH-D<sub>3</sub> only by the kidney<sup>16</sup>. Its identity was finally established in 1971 as 1,25-dihydroxycholecalciferol (1,25-diOH-D<sub>3</sub>, see figure)<sup>17-19</sup>.

1,25-diOH-D<sub>3</sub> is now generally accepted as the bio-

## Biochemistry of Vitamins D



Chemical transformations of vitamin D<sub>2</sub> and D<sub>3</sub> and their principal metabolites. Ergosterol differs only in its side chain from 7-dehydrocholesterol and only this side chain is shown. The same changes are produced in both molecules by ultraviolet irradiation. Only the side chain of 25-hydroxycholecalciferol (25-HCC) and only the lower ring (A-ring) of 1,25-dihydroxycholecalciferol (1,25-DHCC) are shown.

logically active form of vitamin D. It is similar in structure to the steroid hormones and it satisfies very well the criteria for classification as a hormone: it is rapidly active in minute concentrations, as low as  $10^{-10}$  M, in intestine and bone both *in vitro* and *in vivo*<sup>20-23</sup>; recent evidence suggests that it binds initially to a specific cytosol receptor in the intestine<sup>24</sup> and subsequently associates with a chromosomal receptor protein in the nuclear chromatin<sup>14,25</sup>. Early evidence concerning the subsequent course of events was a little conflicting, but there is now good evidence that 1,25-diOH-D<sub>3</sub>, acting in a manner analogous to that of steroid hormones, regulates DNA transcription inducing the synthesis of specific mRNA(s) which then mediate the *de novo* synthesis of calcium binding protein (CaBP)<sup>26,27</sup>. This action of 1,25-diOH-D<sub>3</sub> is inhibited by actinomycin D (ref. 28), as is its stimulation of bone calcium mobilisation<sup>29</sup>. Much more work is required to determine the possible physiological activity of the vitamin D metabolites in muscle, but the clinical evidence for such a specific action is strong.

What factors regulate the synthesis and function of this hormone? The mystery of the adaptation to low calcium diets which occurs in growing animals began to clear when it was shown that young rats respond to this situation by increasing the synthesis of 1,25-diOH-D<sub>3</sub> (ref. 30). It seemed likely at first that this response was mediated through falling concentrations of plasma calcium and this association was then confirmed *in vitro*<sup>31</sup>. Furthermore, kidney tissue was found to secrete a second, apparently inactive, metabolite identified as 24,25-diOH-D<sub>3</sub> (ref. 32) in inverse proportion to the active compound. Because parathyroid hormone secretion is also controlled by variations in plasma calcium the question then arose as to whether parathyroid hormone acts as a trophic hormone for 1,25-diOH-D<sub>3</sub> synthesis, and evidence has suggested such a role. In the experimental animal, parathyroidectomy may abolish 1,25-diOH-D<sub>3</sub> synthesis and administration of parathyroid extract may restore it, opposite changes occurring as expected in the synthesis of 24,25-diOH-D<sub>3</sub> (ref. 33). Corresponding changes in the responsible renal 1 $\alpha$ -hydroxylase activity have also been shown and these changes appeared in advance of the expected changes in plasma calcium which were also produced by parathyroidectomy or by the administration of parathyroid extract<sup>34</sup>. 1,25-diOH-D<sub>3</sub> synthesis in isolated renal tubules was shown to be stimulated by cyclic AMP as well as by parathyroid hormone, and to be inhibited by calcitonin<sup>35</sup>.

Precise theories were soon modified, however, when it became apparent that powerful control of 25-OH-D<sub>3</sub> metabolism could also be exerted by inorganic phosphate. 1,25-diOH-D<sub>3</sub> synthesis, having been abolished by parathyroidectomy<sup>33</sup>, was restored when increasing concentrations of plasma phosphate were reduced by dietary manipulation<sup>36</sup>. Moreover, the ability of the kidney to secrete either 1,25-diOH-D<sub>3</sub> or 24,25-diOH-D<sub>3</sub> correlated with concentrations of both plasma phosphate and kidney cortex inorganic phosphate. At high concentrations of phosphate 24,25-diOH-D<sub>3</sub> was produced, and as phosphorus concentrations were reduced so synthesis switched to the active form. The low serum phosphate produced by low phosphorus-containing diets was found, as expected, to be associated with increased intestinal calcium transport<sup>37</sup>. The independence of 1,25-diOH-D<sub>3</sub> production from that of parathyroid hormone has been confirmed<sup>38</sup>; administration of cholecalciferol alone may

reduce 1,25-diOH-D<sub>3</sub> synthesis<sup>39</sup> and it is open to speculation whether this too is mediated through changes in plasma phosphate. Regulation of 1,25-diOH-D<sub>3</sub> synthesis has in addition been attributed to intracellular calcium concentration in some component of the renal cell<sup>38,39</sup>, but if so, calcium changes in this theoretical compartment are certainly not mirrored by similar changes in plasma calcium concentrations.

The physiological role of parathyroid hormone in control of vitamin D metabolism is therefore uncertain, certainly with regard to human biology and medicine. In the intact animal it is still entirely possible that parathyroid hormone mediates control of vitamin D metabolism by the alteration of plasma and tissue inorganic phosphate which it produces. The species differences, with regard to calcium homeostasis, are fairly prominent and variation with age within any given species may be equally prominent. For example, plasma calcium and phosphate concentrations are much more responsible to dietary manipulation in young animals; an additional mechanism, by which vitamin D metabolism is directly controlled by alteration in plasma phosphate concentrations, could thus be appropriate, at least in early life. Human hypoparathyroidism may be markedly resistant to treatment with vitamin D, a finding which is consistent with a requirement for parathyroid hormone in normal vitamin D metabolism. Although phosphate depletion alone is a very rare cause of osteomalacia in humans<sup>40</sup>, the type of osteomalacia it produces is almost unique in its association with greatly enhanced calcium absorption and marked hypercalciuria<sup>41</sup>. May this not also be due to the stimulated synthesis of 1,25-diOH-D<sub>3</sub>?

Still more mysteries remain. Contrary to earlier beliefs, when 24,25-diOH-D<sub>3</sub> was administered experimentally, a slow but profound stimulation of intestinal calcium transport was observed although bone calcium mobilisation remained unaltered. This effect was the result of its conversion to a still more polar metabolite which localised in the intestinal mucosa and has been tentatively identified as 1,24,25-trihydroxycholecalciferol<sup>42</sup>. If this metabolite has a physiological role, a therapeutic role may await the synthetic compound 1 $\alpha$ -hydroxycholecalciferol<sup>43</sup>. Its relatively easy synthesis has been described and its effects, probably exerted subsequent to its *in vivo* 25-hydroxylation, seem as rapid and profound as administration of 1,25-diOH-D<sub>3</sub> itself.

These advances have done much to explain the well-known association of rickets with diseases of the liver, kidney and intestines. Rare, recessively-inherited diseases, such as pseudo-vitamin D deficiency rickets which alone exactly mimics very severe classical rickets except for the huge doses of vitamin D required for its adequate treatment, may be found to be caused by genetically-determined enzymatic deficiencies, for example of the renal 1 $\alpha$ -hydroxylase. Other genetically-determined diseases may come to be explained on the basis of a structural abnormality, for example in the calcium-binding protein system. Although much of what was confused is now clear, where there was total ignorance there is now controversy. Fortunately the field continues to explode with new knowledge; it cannot be too long before it dispels the clouds of uncertainty which cover the sunshine vitamin and hormone.

T. C. B. S.

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## New Mode of Aerodynamics

It is now well established that most flying insects depend, for their lift and thrust, on conventional aerofoil action which sets up a bound vortex around the moving wing to create a steady-state flow of air and thus provide the necessary aerodynamic cross-force. Weis-Fogh has shown that the same mechanism is used by most insects during hovering flight. But there have been claims that certain insects, notably lamellicorn beetles, must make use of non-steady principles to a large extent. Indeed, because wing movements are oscillatory, and the propeller action is achieved by the twisting of the wing at the end of each upstroke and downstroke, there are necessarily non-steady periods in the flight of all insects, particularly at these reversal points where active pronation and supination of the wings take place. In the August number of the *Journal of Experimental Biology* (**59**, 169; 1973) Weis-Fogh describes a reinvestigation of the capacity of insects for hovering flight which has demonstrated that in certain insects these twisting movements themselves generate aerodynamic forces of a completely novel character which provide the main source of lift.

In studying the flight of the minute chalcid wasp *Encarsia formosa*, which has a Reynolds number of only 10–20 (in place of 100–10,000 in most flying animals), it became evident from calculation that flight could not be explained on the basis of conventional aerodynamics. High speed cinematography revealed that, whatever the orientation of flight, the wings are first raised vertically above the thorax and clapped together in apposition; they are then actively flung apart, like the opening of a book, being pronated with the posterior margins remaining in apposition. Weis-Fogh calculates that this movement, which he terms the 'fling', will set up a temporary vortex

around the wing and provide enough lift to sustain flight. This airflow will die away as the wings separate. But when they reach the lower limit of their excursion the wings are rapidly twisted in the reverse sense and supinated before the upstroke. This twisting movement, called the 'flip', will also set up a vortex around the wing and Weis-Fogh suggests (although exact calculation has not yet been possible in this case) that useful lift is again generated.

This new type of non-steady aerodynamics opens a wide field for future study. Non-steady periods, particularly 'flip' effects, are present in all flapping animals and they will be superimposed on the steady-state pattern. Weis-Fogh shows that the 'fling' operates in butterflies which likewise clap their wings above the thorax; there is preliminary evidence that the 'flip' is used by large hovering dragonflies and by the true hoverflies (*Syrphinae*). It was earlier shown that the hovering of *Drosophila* could be just accounted for in terms of conventional aerodynamics, but it is likely that the new mechanism makes a large contribution to the flight of these very small insects. The flight of insects is largely dependent on the storage of energy, generated in the movement of the wings, within elastic structures of the thorax, to be utilized for the reciprocal movement. There is suggestive evidence that the wing twistings occur so rapidly that the speed of propagation of the elastic torsional wave from base to tip of the wings plays an important part in hovering insects. Non-steady aerodynamics may well be utilized by bats, and the familiar 'clap' of pigeons taking off in a confined space strongly suggests that the 'fling' mechanism has been discovered also by birds.

From our Insect Physiology Correspondent



## GENETICS

**DNA Dominant at Berkeley, California**

from a Correspondent

CHROMOSOMAL organization may still be far from a total solution, but geneticists who attended the quinquennial international congress in Berkeley (August 20–29) went away confident that at least some of the finer details would be resolved by the time they met again in Moscow.

**DNA Redundancy**

The problem of DNA redundancy continues to intrigue several teams, without finally yielding all the secrets of its function or the reason for the wide variation in amount from species to species. Some of the extra DNA is almost certainly present as spacer sequences between cistrons, but this does not account for the large amount of simple sequence DNA, present in millions of copies, in the centromeric heterochromatin. P. M. B. Walker, for whom the Medical Research Council has recently set up a unit in Edinburgh specifically devoted to research on the mammalian genome, reviewed the history of satellite DNA, but said that most investigators would still go no further than suggest that this material, which is not transcribed, has some "housekeeping" function. M. L. Pardue (Massachusetts Institute of Technology) described the results of recent studies on *Drosophila* salivary gland chromosomal DNA and *Xenopus* RNA and showed how hybridization and labelling with quinacrine mustard can serve to distinguish gene and satellite areas. Her conclusion was that satellite DNA has something to do with mitosis or meiosis or both.

A potentially exciting contribution to the congress, and one which was not without relevance to the debate on redundant DNA, was the claim by S. Wolff, S. Abrahamson, M. A. Bender and A. D. Conger (University of California, San Francisco) that the frequency of radiation-induced mutations is directly related to genome size in a wide spectrum of organisms. They took data for mutation rate/locus/rad from various sources, including those of M. Lyon *et al.* (MRC Radiobiology Unit, Harwell) and W. L. Russell (Oak Ridge National Laboratory), and showed that although these figures varied by more than a thousand times, they came well within an order of magnitude when normalized with respect to the DNA content of the nucleus. Whether or not this will serve to illuminate the role of redundant DNA, it will—if true—have a profound influence on the work of those who have to advise on the genetic effects of radiation in man. Recently published levels of risk have been no more than estimates based on the effects

of radiation in *Drosophila* or mouse, and have limited epidemiological data for man. If the claim of Wolff *et al.* is substantiated, however, it will be possible to interpolate the effects in man on the log-log plot of mutation rate against genome DNA—simply by reading off the straight line at the appropriate DNA value. Some participants were quick to point out weaknesses in the proposed relationship. One criticism concerned the fact that several points on the graph, for example, that for the mouse, represented average mutation rates for several loci which in fact differed among themselves by a factor of thirty or more. Clearly the question will be on the minds of many radiation geneticists for some time to come.

The mealy bug came into its own at a session on chromosome inactivation. L. Berlowitz (State University of New York, Buffalo) described possibly the first successful attempt to manipulate artificially an inactivation or reactivation process. On the addition of polystyrene sulphonate, mealy bug heterochromatin decondenses and, on the evidence of increased RNA synthesis, the X chromosome is reactivated. The system will be valuable for studying heterochromatinization in relation to sexual development, as well as the mechanism of chromosomal inactivation generally.

**Sequencing**

The climax of the congress, at least for those geneticists who like their DNA spelled out unambiguously in nucleotide sequences, was a presentation by N. Maizels who, with W. Gilbert (Harvard University), had just completed sequence analysis of the operator region of the *lac* operon. The isolation of pure *lac* repressor first facilitated the purification of double-stranded operator, twenty-seven base pairs long. This was then transcribed by RNA polymerase in forced conditions to yield a series of short RNA sequences from which a picture of the complementary DNA could be built up. The team had then proceeded to the messenger RNA, whose structure holds the key to the region between the operator and the *z* gene. Identification of the initiator codon, AUG, led to a flawless matching of the mRNA sequence with the amino acid sequence of  $\beta$  galactosidase; and matching with the complementary DNA primary structure provided conclusive evidence that transcription is initiated in the operator region.

**Oncogenetics**

A symposium with the title "oncogenetics" provided the opportunity to hear recent advances in viral onco-

genesis, and in particular the application of reverse transcriptase to the search for viral material in human tissue. This session concluded fittingly with S. Spiegelman (Columbia University) demonstrating to a hushed audience, still very much awake at the late hour of 22.45 h, that man does not inherit the genetic material which of itself is both sufficient and necessary for the development of a tumour. Use of reverse transcriptase "probes" has shown that inherited viral material is indeed detectable in certain cancers, but that additional, non-inherited, nucleotide sequences are present in the tumour as well.

In a related contribution, but almost incidentally to his main theme, D. Baltimore (MIT) introduced a new dimension into current thinking about somatic diversification. Baltimore described his work on so-called terminal transferase and other enzymes which seem to have the function of modifying existing polynucleotides. The discovery of terminal transferase in the thymus, and the presence of a similar—though less studied—enzyme in the bursal compartment of the immune system, suggests that such enzymes are responsible for observed differences in the variable regions of the immunoglobulins, and that these differences do not therefore reside wholly in the messenger RNAs. Other examples of protein diversification may be explained in due course if other terminal transferases come to light.

**Mutagenesis**

Not oblivious of social needs, geneticists are turning their attention to the use of bacterial and mammalian cell cultures for the detection of potential mutagens and carcinogens (often one and the same thing). B. Ames (University of California, Berkeley) talked about a mutant of *Salmonella*, with a frameshift mutation affecting a histidine locus, which he has developed for the testing of mutagens on the basis of their capacity to revert the mutation. The sensitivity of the system has been refined (to the  $\mu$ g or even the ng level) by eliminating the normal excision repair system and also by selecting a rough strain, which offers less of a mechanical barrier to the mutagen under test. Ames claims that such a bacterial system should give a good indication of mutagenicity in mammalian cells, but it is quite feasible to add a homogenate of mammalian tissue to allow for the effect of normal detoxifying systems and so make the method more immediately relevant to mutagenesis in man.

The organizers had wisely allotted time in the programme for sessions on the social impact of genetics—the questions of genetic engineering, race and IQ, the Green Revolution—but these

fell rather flat. Participants displayed a studied dissociation from genetic engineering; and in the absence of Jensen or any of his followers, discussion of the hereditary element in intelligence was cool (in spite of the presence of activists handing out leaflets and seeking signatures for petitions). Only the Green Revolution—the doctrine of high yielding monoculture farming as the only salvation for the developing countries—sparked a really lively discussion. Some referred to the impairment of the genetic pool and the risk of disease inherent in monoculture methods; others went so far as to suggest that Borlaug's work on the development of high yielding seed strains, for which he was awarded a Nobel prize, was all part of a capitalist plot to dominate world grain markets. Most participants, however, were happy to return to less controversial sessions—on genetic polymorphisms in the mouse or the behavioural genetics of *Drosophila*.

#### JUVENILE HORMONE

### Hypothesis of Action

from our Insect Physiology Correspondent  
It is well established that the activity of natural juvenile hormone, or of the various "mimics" of juvenile hormone, when introduced experimentally into the insect, is largely determined by the rate of hormone breakdown. In the case of the natural hormone, breakdown consists of (1) hydrolysis of the terminal ester linkage by an esterase to form a carboxylic acid and (2) cleavage of the epoxide ring by an epoxide hydase to form a dihydroxy derivative. Both these types of endproduct are known to be inactive. Slade and Wilkinson (*Science*, **181**, 672; 1973) have now obtained enzyme preparations from the alimentary canal of the army worm caterpillar *Prodenia eridania* and they show that the breakdown of juvenile hormone by these preparations is inhibited by various juvenile hormone mimics. It is perhaps not surprising that the epoxide hydase activity is inhibited particularly by compounds with epoxide groups, and the esterase by compounds with terminal ester linkages.

The authors infer from these observations that compounds of this type are not in fact reproducing the activity of the juvenile hormone, but are merely inhibiting the breakdown of the hormone already present in the organism. They describe the more usual interpretation as "a case of mistaken identity". They find their interpretation attractive because the apparent absence of any common structural features has been difficult to rationalize in terms of their interaction with a specific hormone receptor. Some years ago W. S. Bowers put forward a somewhat similar view,

to the effect that synergists and mimics of juvenile hormone activity act by inhibiting the metabolic breakdown of ecdysone and thus upsetting the hormone balance.

Biologists, on the other hand, regard the action of the juvenile hormone as being the result of gene switching. They are familiar with the fact that very striking switches of this kind can be brought about by widely different chemicals, just as very different chemical substances may have identical odours or tastes. Slade and Wilkinson are not impressed by the fact that juvenile hormone mimics are effective in insects deprived of their source of juvenile hormone by removal of the corpora allata.

#### GEODESY

### World Triangulation

from a Correspondent

ONE of the aims of geodesy is to unite the triangulation systems of the world into a single, homogeneous network, and the current state of progress towards this objective was reviewed at an international symposium on computational methods in geometrical geodesy held at the University of Oxford from September 3 to 7. The two main themes that were considered were the methods of combining satellite observations with terrestrial measurements in order to provide the necessary transcontinental and intercontinental connexions, and the solution of the resulting very large systems of normal equations.

R. J. Anderle (Naval Weapons Laboratory, Dahlgren) described how Doppler satellite observations have been used to determine the positions of thirty-seven points distributed over the Earth's surface, with a standard error of about

2.5 m and with an estimated scale error of 1 in 10<sup>6</sup>. From these Doppler positions the datum shifts required to transform the latitudes, longitudes and heights of points referred to the twenty-six separate national or continental triangulation origins into three-dimensional coordinates in a global geocentric reference system have also been obtained.

The results are compatible with, but somewhat superior to, those given by a similar exercise using camera observations of the PAGEOS balloon satellite. Doppler and camera observations are, however, not the only methods of using satellites, and F. J. Lerch (Goddard Space Flight Center, Greenbelt) and J. S. Reece (Computer Sciences Corporation, Falls Church) gave details of a proposed method of combining camera observations, dynamic satellite observations (including electronic, laser and optical tracking data), gravity measurements and long measured base lines on the Earth to produce a geocentric system of station coordinates, together with spherical harmonic coefficients for the geopotential.

Networks of this type give rise to very large sets of linear equations, of order 7,000 or more, and although the numerical solution of such systems no longer constitutes a mathematical problem, it is a topic of continuing interest to geodesists, for, in addition to the numerical values of the unknowns, the solution must also provide information on the accuracy with which these values have been determined, on the accuracy of the observations, and on the stability of the system. These requirements generally demand the inversion of the matrix and the determination of at least the maximum and minimum eigenvalues; the practical aspects of such determinations were discussed in con-

### Test-tube Replicase

WHEN the RNA bacteriophage Q $\beta$  infects *Escherichia coli*, its genetic material acts as messenger RNA and, among other proteins translated, is the R protein which is the phage's contribution to the enzyme replicase. Three proteins, provided by the host, complete the active enzyme. Replicase is an RNA-dependent RNA polymerase and is required for the reproduction of the virus.

Phage proteins are synthesized when Q $\beta$  RNA is added to an *E. coli* cell-free system and presumably include R protein, but until the work of Happe and Jockusch, described in *Nature New Biology* next Wednesday (October 3), active enzyme had not been demonstrated in the cell-free mixture. These authors think this is because replicase must be inhibited by the large amount

of Q $\beta$  RNA added as messenger and, before attempting to show its ability to act in RNA replication, they have first isolated the enzyme from the protein synthesis mixture.

During cell-free protein synthesis R chains are produced which are not incorporated into complete enzyme. In fact, only about 4% of R molecules form active replicase. Once formed, however, the *in vitro* enzyme resembles the native one in all ways tested. It has the same sedimentation coefficient, contains all three host subunits, can use poly (rC) or Q $\beta$  RNA as template and has roughly the same specific activity as the *in vivo* enzyme.

This communication describes one further step in the attempt to reconstruct the replication of Q $\beta$  in cell-free conditions.



tributions by J. F. Dracup and J. G. Gergen (National Geodetic Survey, Rockville), G. Schmitt (Technical University, Karlsruhe) and D. Ehlert (Institut für Angewandte Geodäsie, Frankfurt).

From the progress reported at this symposium, it is clear that a world triangulation system lies but a short step away.

## EARTH'S MAGNETIC FIELD

### Dipolar Reversals

from our Geomagnetism Correspondent

IN spite of the large number of investigations into reversals of the Earth's magnetic field, the question of precisely how the field behaves during a transition from one polarity to the other has not yet been answered satisfactorily. The two extreme possibilities are that the dipole decreases in strength to zero and then builds up in the opposite sense while maintaining its axial or near-axial form, or that it simply rotates through  $180^\circ$  with no change of magnitude. Both of these modes of behaviour are probably consistent with the self-exciting dynamo hypothesis for the origin of the main field, although, because the dynamo involves fluid core motions, the dipolar decrease-increase is intuitively more likely than the rotation. But as neither type of transition seems to be completely excluded on theoretical grounds, it is also possible to conceive of a third possibility—a rotation of the dipole combined with a decrease in strength (but not necessarily to zero).

For many years there seemed little reason to doubt that transitions were generally of the simple decrease-increase type, especially as this behaviour appeared to be more in accord with a fluid dynamo. Unfortunately, the situation is complicated by the existence of the non-dipole field which seems to act quite normally during a reversal. Thus even if the dipole decreases in strength to zero, the total field does not; and so palaeomagnetic data from rocks formed during transitional periods may still be represented in terms of apparent palaeomagnetic poles. If only the non-dipole field were present over much of a transition, the transition poles from many reversals would be expected to be randomly scattered throughout all longitudes and latitudes. But some recent analyses have hinted that there may be preferred meridional bands within which most, or at least many, transition pole paths lie. As a result, there has been some incentive to reconsider the question of whether the dipole may rotate, with or without a corresponding variation in magnitude.

The chief practical difficulty with this type of work arises because transition periods between polarities, being of the

order of  $10^4$  yr, are much shorter than fully normal or reversed epochs. Thus only a few per cent of rocks were magnetized during transitions, there are only a few known examples of more than one or two igneous bodies within any single transition, and no single transition has yet been found to produce a palaeomagnetic record in more than one locality. Nevertheless, with imperfect and limited data from twenty-three Holocene, Tertiary and Upper Mesozoic transitions, Steinhauser and Vincenz (*Earth planet. Sci. Lett.*, **19**, 113; 1973) have been able to throw important statistical light on to the nature of field reversals.

The first step in their investigation was to analyse the longitudinal frequency distribution of transitional palaeopoles lying between  $40^\circ$  N and  $40^\circ$  S (poles usually referred to as "intermediate"). Because some transitions are represented by a single pole, others by a series of poles forming a clear pole path and yet others by a cluster of pole positions, each transition was reduced to a single pole with an average longitude for the purpose of this analysis. The resulting frequency distribution of the 23 longitudes was bimodal with peaks at longitudes  $40^\circ$  E ( $140^\circ$  W) and  $60^\circ$  W ( $120^\circ$  E) and no values at all in the ranges  $6^\circ$  E– $35^\circ$  W and  $85^\circ$  E– $70^\circ$  W. In other words, during transitions the longitudes of the apparent pole positions appear to lie preferentially within two meridional bands centred on the planes through  $40^\circ$  E– $140^\circ$  W and  $60^\circ$  W– $120^\circ$  E, respectively. Statistical tests confirm that the frequency distribution differs significantly from a random distribution.

The conclusion that Steinhauser and Vincenz draw from this pattern is that, quite apart from any possible variation in strength, the dipole may flip over during a transition from one polarity to another. Moreover, further details of the nature of this rotation may be obtained from the corresponding latitude frequency distribution which is also significantly non-random. For palaeopoles lying between  $40^\circ$  N and  $40^\circ$  S, the distribution is U-shaped with the number of latitude values decreasing to a minimum at the equator. This is certainly not consistent with the existence of only a non-dipole field. But it is consistent with a dipole which not only rotates but speeds up when its axis reaches lower latitudes. The latitude frequency distribution suggests that the relative lack of palaeopoles near the equator occurs because the dipole is rotating three to four times faster in equatorial latitudes than it is at  $40^\circ$ .

Dagley and Wilson (*Nature phys. Sci.*, **232**, 16; 1971) have found that as the geomagnetic pole moves away from the geographic pole the strength of the dipole decreases; and Steinhauser and Vincenz accept that this decrease occurs

simultaneously with the rotation. They argue, however, that in view of the speed-up of the dipole in lower latitudes, a plot of ancient field intensity against the latitude of the palaeopole over-emphasizes the importance of the dipole's decrease in strength. Combining the field intensity data with the latitude frequency distribution, Steinhauser and Vincenz show that the dipole is reduced in strength by about one order of magnitude for about 12% of the transition time. During the rest of the time the dipole intensity is comparatively high, and for at least two thirds of the time is higher than typical non-dipole field intensities.

## GAMMA RAY SPECTROSCOPY

### Line Narrowing

from a Correspondent

THE widths of the spectral lines of gases are usually determined by Doppler broadening. The cause of this inhomogeneous broadening, the Doppler effect, is that if an emitter has a velocity component  $+v$  or  $-v$  along the direction of propagation of the radiation, then the frequency of the light observed appears to be displaced to higher or lower frequencies respectively by  $v/c$ , where  $c$  is the velocity of light. The intensity of each component of the line is given by the number of molecules with a definite component of velocity relative to the observer and is normally given by a Maxwellian distribution of velocities, so that the observed line has a Gaussian profile. The advent of gas lasers has made possible the selection and saturation of a particular velocity group of molecules so that a "hole is burned" in the Maxwellian velocity distribution. This perturbation of the population of the molecules can be detected by several types of experiment, both in emission and in absorption, as a narrow trough or peak on the broad Doppler background. The width of this saturation-induced feature is usually much narrower than the Doppler width, and is related to the homogeneous broadening associated with the natural line width, pressure broadening and power saturation.

Most of the applications of this type of experiment have been confined to the infrared and visible spectral regions, where they have been exploited in the new determination of the velocity of light (Blaney *et al.*, *Nature*, **244**, 504; 1973). Recently, however, Letokhov, of the Institute of Spectroscopy, Akademgorodok, one of the pioneers of this type of work, has proposed an extension of the idea to  $\gamma$ -ray spectroscopy (*Phys. Rev. Lett.*, **36**, 729; 1973). The suggested method of line narrowing requires the use of a laser which will excite a vibrational mode of a molecule containing an atom which is a  $\gamma$ -ray

emitter or absorber. As the vibrational amplitudes of most atoms in molecular vibrations are of the same order of magnitude as the wavelength of  $\gamma$ -ray quanta with an energy of 10 keV to 1 MeV, the vibration of the nuclear emitter should cause a considerable frequency modulation of the  $\gamma$  radiation. In order to observe this effect the nuclear emission or absorption must be observed along the direction of propagation of the laser radiation. The nuclear absorption or emission line should be distorted by the interaction of the molecule with the intense laser radiation so that a dip is produced close to the centre frequency of the  $\gamma$  resonance, but shifted slightly from the centre by the effects of nuclear recoil and the particular velocity group chosen.

In addition to the dip, "sidebands" are produced at multiples  $m$  of the laser frequency, and these features should consist of emission peaks. The number of these "sideband" peaks depends on the amplitude of the nuclear vibration excited, and in the simplest example when  $m=1$  there will be two peaks symmetrically placed about the dip. The widths of these resonances should be  $10^{-2}$  to  $10^{-4}$  of the Doppler width and they should be tunable by up to  $10^{-5}$  of the  $\gamma$  frequency. Because only a small fraction of the molecules in the gas are in resonance with the coherent light wave, the amplitude of the resonances will lie in the region  $10^{-1}$  to  $10^{-3}$  of the total amplitude of the Doppler-broadened transition.

This picture is complicated by the effects of molecular rotation which will broaden the resonances considerably, although they will still be much narrower than the Doppler width. In order to minimize the effects of rotation it is suggested that it is better to choose molecules with the nucleus being studied on one of the principal rotation axes, or large molecules which have small rotational constants. As the resonant absorption coefficient for nuclear transitions in a gas at the low pressures needed for saturation is very small, it should be possible to observe the narrow resonances in absorption due to the  $\gamma$  fluorescence of the gas, or in the emission from a gas containing  $\gamma$ -active nuclei.

The use of this method for studying phonon absorption spectra of solids in the wing of a nuclear absorption line has also been suggested, allowing the measurement of the recoil energy of nuclei with an accuracy much greater than at present because the laser tuning can be measured very accurately. This method may allow the measurement of  $\gamma$ -quantum energy to an accuracy of  $10^{-8}$  and a comparable accuracy in the connexion of the energy scales of the optical and  $\gamma$  ranges.

## NUCLEAR PHYSICS

# Complications Continue

from a Correspondent

At the International Conference on Nuclear Physics (successor to the 1969 Montreal conference) which was held in Munich from August 27 to September 1, it was clear that nuclear structure and reactions continue to turn up surprises and complications in abundance. Yet just two examples of developments in reaction theory indicate that phenomena which do not fall into one or other of the simpler extremes are no longer being ignored.

G. R. Satchler (Oak Ridge National Laboratory) spoke of the need to recognize multi-step processes in direct reactions and of the success of coupled channel calculations in accounting for them. Of course such processes, which are virtual transitions via intermediate states, must exist but, as Satchler showed with examples of (p, t) and charge exchange reactions, they may be comparable with or stronger than the one-step (DWBA) processes. J. M. Miller (Columbia University) discussed the theoretical efforts being made to understand pre-equilibrium processes — reactions which in time scale fall between the prompt (direct) and compound nucleus extremes, that is, the projectile suffers several but not a statistically huge number of interactions within the nucleus before the reaction products emerge. Examples were shown of efforts to reproduce the observed spectra of protons emerging from (p, np) reactions on heavy nuclei although it seems there is still much left to learn.

The increased availability of heavy ion beams and improvement in particle identification techniques has facilitated a vigorous effort in multinucleon transfer processes, reviewed by D. K. Scott (University of Oxford). The significant improvement in quality and quantity of data and the selectivity of the process in populating nuclear states has now established this field especially in the study of cluster structure in nuclei. D. Pelte (University of Heidelberg) spoke of the structures seen in excitation functions in systems like, for example,  $^{12}\text{C}+^{12}\text{C}$ ,  $^{12}\text{C}+^{16}\text{O}$ ,  $^{24}\text{Mg}+^{24}\text{Mg}$ , both above and below the Coulomb barrier. It is both interesting and puzzling that gross structures, which may correspond to quasimolecular resonances, are seen in some systems but not others. In the general area of resonant reactions there were several contributions reporting possible intermediate structure effects, but a convincing identification of these is not always easy and more objective statistical tests, such as those mentioned by Y. Baudinet-Robinet and C. Mahaux

(University of Liège), might be used in data analysis.

H. J. Specht (Ludwig-Maximilians University, Munich) presented some of the systematics which have been gathered on fission isomers. The explanation of these in terms of a double-humped barrier seems now established and its verification has stimulated arguably some of the best recent experimental work. Indeed the physics of fission probably commands more attention now than for some time.

F. Stephens (University of California, Berkeley) presented evidence of decoupled bands in odd-A nuclei in the mass region of the deformed heavy nuclei. These bands may be formed by the weak coupling of an odd nucleon to the rotational (strong coupling) states of the neighbouring even nucleus and could arise, according to Stephens, from a cancellation in the Hamiltonian for a particular deformation. The oldest band in the business, the 0,2,4... notational sequence, continues to attract interest, not only through the familiar "backbending" phenomenon, for which no explanation is yet generally accepted, but also in the more recently reported "forking". This is the splitting of the ground state band into two which has been observed at a spin value of 8 in two palladium isotopes.

Fields related to nuclear physics provided some interesting talks. J. N. Bahcall (Princeton University) spoke of the impasse in the solar neutrino problem. At present nobody can see any flaw in the nuclear physics and stellar evolution theory which together predict a certain minimum flux on Earth of solar neutrinos or in the one extraordinarily careful experiment which has so far failed to detect it. If nuclear physics is central to stellar physics it is evidently now more than touching archaeology, and I. Perlman (Jerusalem) showed how firm evidence concerning the origin of clay used in ancient pottery can be found by neutron activation analysis.

This correspondent feels bound to raise some questions concerning the scope of a conference of this nature and magnitude, notwithstanding its value to the very many participants. It seemed that a few of the talks did less than justice to the fields being reviewed. Perhaps it really is becoming too difficult — at least in less than an hour — both to review a branch of nuclear physics to the interested non-specialist and also to discuss current problems and new directions for the researcher more clearly involved. It will surely continue to be essential to have periodic meetings surveying the whole of nuclear physics. It may be, however, that these should be fractured into fewer pieces and have the primary aim of helping participants perceive the unifying threads of the subject.



# Changing Attitudes towards Science among Adolescents

ANDREW AHLGREN & HERBERT J. WALBERG\*

Harvard University

**Ratings of students reveal an unfavourable image of physics and possible directions for improvement.**

DURING the past decade or so, concern has been growing among science educators about the decreasing proportion of high school students who elect to take a course in introductory physics. Exact figures are difficult to obtain<sup>1</sup>, but it seems safe to say that the percentage of high school students taking physics has dropped from about 25% in 1950 to less than 20% in 1970 (refs. 2 and 3). However one gauges the loss to liberal education, this decline may be of concern to scientists in general because of its implications for the willingness of young voters and taxpayers to support science.

## Role of Teacher

Some of the unpopularity may be attributable to science teaching rather than to the nature of the subject itself. It is undeniable that a great many high school physics teachers are poorly prepared. Particularly good documentation is available for Iowa, by virtue of the Iowa Public Schools Employee Data Sheet, which all teachers in the state must submit annually. A compilation made in 1965–66 (ref. 4) showed that the number of teachers with less than 20 semester hours in their science specialty was 13% for biology teachers, 39% for chemistry teachers and 63% for physics teachers. In a random sample of fifty-four teachers from the National Science Teachers Association list of more than 16,000 high school physics teachers in the continental United States<sup>5</sup>, physics background ranged from 8 to 59 semester hours, with a mean of 23.4 hours. The data from this sample showed, however, no significant relationship between teacher background and the attitudes of students toward physics<sup>6</sup>. In a study of forty-eight high schools in Michigan, Van Koevering<sup>7</sup> found no relationship between the physics training of teachers and the percentage enrolment of students in physics.

A more likely cause of the unpopularity of the high school physics course is the severity of grading practices of many teachers. Although physics students are clearly a select group (with a mean IQ of 116 in our national random sample), they tend to obtain lower grades in physics than in their other high school courses. Krieger<sup>8</sup> compared grades received in high school physics and chemistry by 172 students who had taken both; he concluded that "the most important result of this study is the recognition that A and B chemistry students are discouraged from taking physics because their physics grade will be lower by three letter grades". On a much grander scale, the journal *Physics Teacher* ran a national survey in 1964–66 (ref. 9); of 1,382 replies from physics teachers, 942 gave as first or second choice as the cause of declining enrolment the statement: "Student fears

grade average will suffer and college chance will be damaged." In Van Koevering's Michigan study<sup>7</sup>, the physics grades of students were found to be generally lower than their grades in other academic subjects, regardless of whether the schools had high or low percentage enrolment in physics.

Bridgham and Welch<sup>10</sup> adjusted the mean final grades assigned by a national sample of teachers for the mean post-course scores on a standard physics achievement test, to give a measure of "severity" of grading; a severe grader would be a teacher who assigned lower grades to his students than would be expected from their achievement compared with other physics students in the country. Grading severity showed a significant negative correlation of  $-0.73$  with student interest in continuing the study of physics.

Thus, as had been speculated, physics teachers seem to be among the most severe graders—and students seem to be markedly discouraged by severe grading. The elitist stance of many physics teachers is unambiguously captured in a remark that appeared in a letter to the editor of *Physics Teacher*<sup>11</sup>: "Teachers do not choose to betray the physics profession by purveying something less than physics in courses bearing the name. The question properly can be asked whether the physics profession should be concerned about physics for those disinclined or unable to devote the desirable intellectual endeavor."

## Attitudes

Surely, however, much of the aversion for physics courses may stem from students' perceptions of the nature of physics and physicists. Several investigators have found negative attitudes towards science and scientists in general and these attitudes seem not to be changing with time or geography. Almost fifteen years ago, Mead and Metreux<sup>12</sup> analysed several thousand short essays on science from a national sample of some 35,000 high school students. The essays were completions of three stem forms:

- (1) "When I think about a scientist, I think of . . ."
- (2) "If I were going to be a scientist, I should like to be the kind of scientist who . . ."
- (3) "If I were going to be a scientist, I would not like to be the kind of scientist who . . ."

(Girls were offered the option of using an alternative stem beginning for (2) and (3): "If I were going to marry a scientist . . .")

The investigators drew from the results a composite picture of the scientist in which he was seen as intelligent and dedicated, and important to the purposes of man. The composite image also includes the following phrases:

"His work is uninteresting, dull, monotonous, tedious, time consuming. . . . He may live in a cold-water flat; his laboratory may be dingy . . . he is so involved in his work that he does not know what is going on in the world. He has no other interest and neglects his body for his mind. He can only talk, eat, breathe and sleep science. He neglects his family—pays no attention to his wife, never plays with his children. He

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has no social life, no other intellectual interest, no hobbies or relaxations. He bores his wife, his children and their friends—for he has no friends of his own or knows only other scientists—with incessant talk that no one can understand. . . . No one wants to be such a scientist or to marry him."

In brief, science was respected for its benefits, but the scientist and his work were seen as unpleasant. In 1961, Beardslee and O'Dowd<sup>13</sup> found a similar dichotomy among some 1,200 college students who rated different occupations on fifteen attitude scales of the "semantic-differential" type<sup>14</sup>. These college students seemed to perceive scientists to be, as the subtitle of the report expresses it, "intelligent and hard-working, but also as uncultured and not interested in people". In 1967, Hudson<sup>15</sup> assessed attitudes of 390 English school boys of high academic ability (aged 13 to 17) with ten semantic-differential scales. He concluded: "A clear implication of

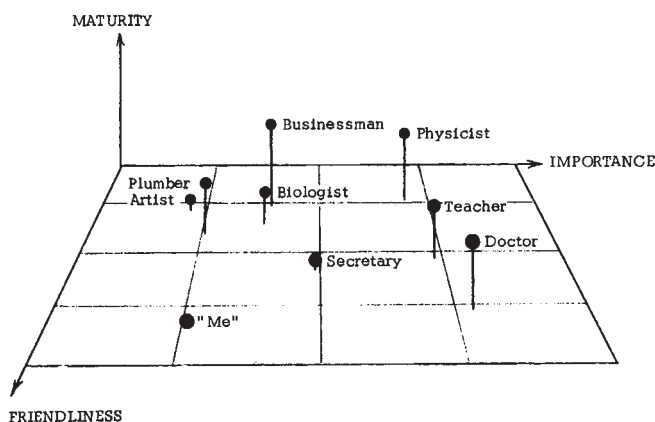


Fig. 1 Mean positions of eight professions and self in three-factor space of student ratings. "Physicist" is the most remote from "me", and "biologist" falls roughly midway between them.

these data is that adult scientists are seen by both future arts and science specialists as leading dull personal lives." In 1970, Mitias<sup>16</sup> found among 290 non-science-major undergraduates that the second most common element in completing the statement "I conceive science . . ." was "a necessary evil". That same year Mackay<sup>17</sup> found in an evaluation of the new State physics curriculum in Victoria, Australia, that after two years in the high school physics programme the students saw scientists as being "less like other people" than they had before the course. And in 1971, McNarry and O'Farrell<sup>18</sup> reported a study of seventy-nine urban and 161 rural students in senior high school, in which students regularly viewed scientists as "helpful, wise and important", but also as "hard, old, frightening and colorless."

In our own first studies of attitudes towards science, we asked ninety-six high school physics students to rate each of eight different occupations. They marked attitudes on fifteen semantic-differential scales, each scale consisting of five levels between a pair of words such as valuable-worthless, friend-enemy, joyful-gloomy. A variety of occupations were used—businessman, artist, teacher, plumber, biologist, secretary, physicist, doctor—in order to establish the semantic space in which students rated occupations. Also included among the occupation was "me". A factor analysis of the responses indicated that about half of the variance among the fifteen ratings on the nine items could be accounted for by just three independent composite scores. On the basis of the scales with the greatest weights in each factor score, we named the factors "importance" (for example, the scales important, valuable, productive), "friendliness" (for example, the scales kind, friendly, joyful, sociable), and "maturity" (for example,

the scales mature, old, hard, boring). The mean rating for each occupation can be plotted in the three-factor space, as shown in Fig. 1. "Physicist" was the most remote from "me"—he was perceived as being very important and mature, but very unfriendly.

The mean rating of "biologist" placed him about midway between "physicist" and "me"—he was rated more friendly, but less important and mature. The specific occupation of physicist thus seems to share the common image of science and scientists. Indeed, on the basis of the relative ratings of "physicist" and "biologist", the physicist seems to be seen as even more remote than scientists in general.

## Comparisons among Courses

These findings led us to pursue the unpleasant image of physics further, using data from classes of the national random sample of high school physics teachers already mentioned<sup>5</sup>. The teachers in the random sample were randomly assigned to teach either a new course (Harvard Project Physics) or the course they had been teaching previously. The question shifted slightly, from "What do students feel about physics?" to "What is it about physics that makes them feel that way?" Near the end of the physics courses, some 1,011 students of these teachers (a random half of each class) rated the concept physics on a set of fifteen seven-point bipolar scales of the type "not theoretical—extremely theoretical". We then calculated mean ratings for the students of each teacher, and compared these means for the different courses. Courses were identified as either of two developed

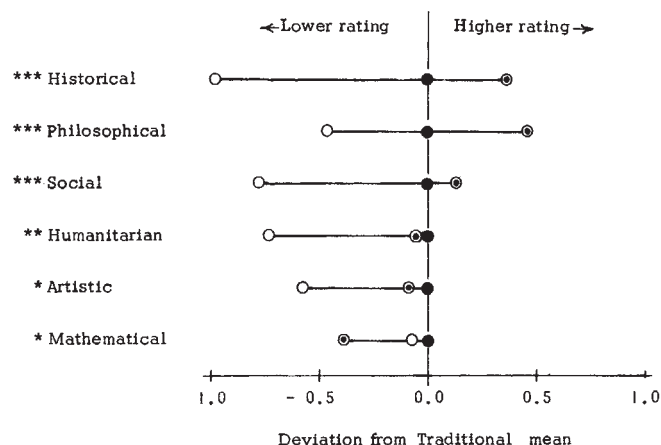


Fig. 2 Relative ratings of physics on the six scales for which there were significant group differences. The mean ratings by students in traditional courses are used as central references, and the means for the students in PSSC and HPP courses are plotted relative to them. The metric for each scale is the pooled within-group standard deviation for that scale (which is for all scales approximately 1/2 scale division on the seven-point rating scale). ●, Traditional classes (151 students of thirteen teachers); ○, PSSC classes (191 students of six teachers); ◐, HPP classes (669 students of thirty-four teachers). \*  $P < 0.05$ ; \*\*  $P < 0.01$ ; \*\*\*  $P < 0.001$ .

by government-sponsored curriculum projects—the Physical Science Study Committee (PSSC) or Harvard Project Physics (HPP)—or any other course ("traditional"). Because of the random sampling procedure, differences among courses may legitimately be inferred to the population, which is about half of the entire set of high school physics teachers in the United States.

A multivariate analysis of variance<sup>19</sup> revealed that the probability of the entire set of group differences on the fifteen scales having occurred by chance alone is less than 0.0001. Figure 2 shows the mean ratings on those scales for which the univariate probabilities of the group differences having



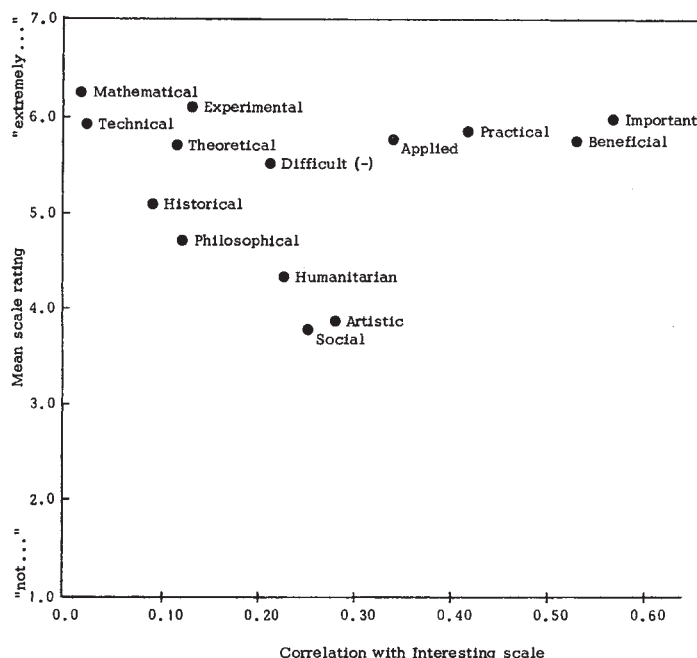


Fig. 3 Mean ratings of physics on fourteen scales compared to correlations of the ratings with the "interesting" rating. On the scales that have the greatest correlations with "interesting", physics is already rated very high. More potential for improving interest in physics may therefore lie with the scales artistic, social, and humanitarian, which correlate only moderately with "interesting", but on which physics has much room to increase.

occurred by chance alone are less than 0.05.

Figure 2 shows that the newest course, Harvard Project Physics, led to the rating of physics as significantly more historical and philosophical and significantly less mathematical. These results are entirely consistent with the intentions of the HPP authors, who set out to make physics more palatable to a larger audience by stressing historical and philosophical context of ideas in physics, and by reducing the mathematical demands on the students<sup>20</sup>. On the other hand, Fig. 2 shows that PSSC, the first national curriculum project in science, led students to view physics as even less historical and philosophical than it was in the traditional courses. This is not unreasonable, since the authors of PSSC intended first of all to increase the accuracy and integrity of the reasoning in the physics course<sup>21</sup>. (The PSSC gave as the second of two chief goals: "to develop a physics course that emphasizes the essential intellectual, aesthetic, and historical background of physical science". But the specific objectives immediately following the goals make no mention of these cultural aspects.) The PSSC course seems, however, to have produced a view of physics as significantly less social, humanitarian, and artistic than the view shared by the HPP and traditional courses. Such a view was not intentionally fostered by the PSSC authors; yet the result is not surprising if their primary goal was to clean up the traditional textbook clutter and write a purer story line of physics theory.

Clearly, then, the contents of a physics course can have marked effects on the students' view of physics. Harvard Project Physics was able to draw on the completed work of the Physical Science Study Committee and, going off in a different direction, apparently succeeded in restoring the social, humanitarian, and artistic aspects that had been lost in PSSC, and succeeded in augmenting the historical and philosophical aspects.

## Implications for Increasing Interest

What does such a new direction bode for student interest in studying physics? The general correspondence of the HPP effects to the Aquarian emphasis on the humanities may be

encouraging. More concretely, the correlations of these aspects of physics with interest in the subject are displayed in Fig. 3, in which the data for all 1,011 students have been pooled. There are plot points for each of fourteen of the rating scales: the vertical axis is the mean scale rating of physics (on a seven-point scale), and the horizontal axis is the correlation of that scale rating with the rating of physics as "interesting". Obviously the ratings most strongly related to interest are "important" and "beneficial". Least related to interest are the ratings "mathematical" and "technical". But for a whole band of scales—from "mathematical" and "technical" through "experimental", "theoretical", "applied", and "practical" to "important" and "beneficial"—there is very little room for increasing the rating. The mean ratings of physics on these aspects are already almost all the way toward the "extremely..." end of the scale.

If one were to choose a single image component to feature in instruction, it would probably be "beneficial", which has nearly the highest correlation with interest, and is not yet as "saturated" as the other high-correlation scales. Perhaps gains could be realized also by giving attention to improving ratings in the cluster of scales "humanitarian", "social" and "artistic". Although these views of physics are only modestly correlated with interest, there is a lot of room to improve them. This suggestion is given further support by the fact that only about 25% of high school physics students are girls. Although the mean ratings by boys and girls are almost identical, the correlations with interest are higher for girls on the scales "humanitarian", "social", and "artistic"—and distinctly lower on the scale "practical" and "applied". (Mead and Metreux<sup>12</sup> stated among their conclusions that "The girls, when they react positively [to science and scientists], emphasize humanitarianism.") Thus the largely untapped audience of females makes the potential of the humanistic components even more attractive as a direction for improvement.

It is interesting to note that 91% of the 1,382 physics teachers who replied to the *Physics Teacher* survey in 1965 (ref. 9) agreed with the statement "Some physics of a cultural kind is needed by nearly everyone." Evidence of just how much room there is in physics courses for enhancing the humanist component is given by a recent study of physics teachers in California<sup>22</sup>, for which the investigators report:

"Although 94% of the teachers perceived their physics course as preparing students for understanding the role of science in society, only 4% reported that they included more than a few of the historical aspects of physics in their course, only 37% reported that they included more than a few of the political aspects of physics, and only 50% reported that they included more than a few of the social aspects in their course."

This study was made on more than half of the public school physics teachers in California. The sampling used made it likely that the results represent the behaviour of the better teachers.

In summary, we can do little better than to quote Bentley Glass's opening paragraph in a recent editorial<sup>23</sup>:

"It is undeniable that our science curricula have in part failed, even the newest and best of them, to deal sufficiently with the role of science in the making of human culture, with the problems of the present world and the fair or dread vision of the future of man."

It seems that the image of physics tends to have strong negative components, that curriculum can affect its image, and that its image is related to interest in studying it. The relation of the science to people—through being beneficial, social, artistic and humanitarian—seems to offer the best chance to regain lost interest and a lost audience.

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# Rainfall Patterns and General Atmospheric Circulation

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This article presents an analysis of the rainfall records of the Mediterranean and the Middle East and of the Sahel zone and north-west India from the beginning of this century and relates rainfall fluctuations to changes in the general circulation of the atmosphere. Probable rainfall trends during the past 1,000 yr are outlined and rainfall trends projected to the year 2030.

I HAVE previously shown<sup>1</sup> that since about 1960 summer monsoon rainfall in the Sahel zone to the south of the Sahara and in north-west India has been highly negatively correlated with winter-spring rainfall to the north of the Sahara, along the Mediterranean coast and in the Middle East. During the 1960s the circumpolar vortex expanded and the meridional circulation in the tropics contracted, giving heavy winter-spring rainfall over the Mediterranean and the Middle East and low monsoon rainfall in the Sahel zone and in north-west India, leading to the present severe drought in the latter areas.

## Rainfall Analysis

The period of records, the rainfall season, and the mean and range of seasonal rainfall at the stations used in my present analysis are shown in Table 1.

Although there were relatively few stations recording rainfall at the beginning of the century, the rainfall trends obtained from the smaller samples are considered representative of trends throughout the zones. This is justifiable, because rainfall trends in the winter-spring rainfall belt are very similar at widely separated stations with mean seasonal totals ranging from 24 to 893 mm (ref. 1). Similarly, although there are no rainfall data for the Sahel zone of west Africa for the first 30 yr of this century, Fig. 1 shows that the trends for Bikaner, Jodhpur and Khartoum can be considered representative of the trends across the entire

zone. Atar (Mauritania) and Bikaner and Jodhpur (Rajasthan, India) are near the northern limit of the summer monsoon rains and the southern limit of the winter-spring rains; seasonal totals have, therefore, been analysed separately and are included in the respective analyses (Table 1).

Crude area-mean rainfall figures for each belt were obtained by expressing the seasonal rainfall at each station as a percentage of the long period average for that station, and then calculating the mean percentage of all the stations. Smoothing was achieved by first computing 5-yr running means, and then 20-yr running means of these values (Figs 2 and 3).

In both rainfall belts there are irregular fluctuations superimposed on longer term trends. Winter-spring rainfall over Africa north of the Sahara, the Mediterranean and the Middle East is highly significantly correlated with summer monsoon rainfall in the Sahel and in north-west India; the correlation coefficient for the individual season area-mean values (not shown in Figs 2 and 3) is  $r = -0.58$ . The 20-yr running mean values show an inverse relationship to each other (Figs 2 and 3). From the beginning of the century, winter-spring rainfall decreased to a minimum of 16% below average in 1926-27, 1927-28, whereas

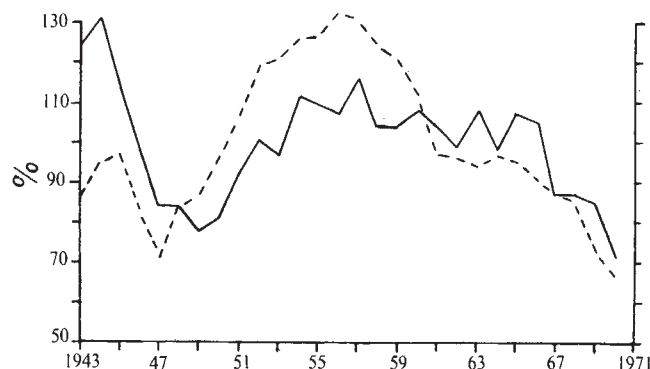


Fig. 1 Five-year running means of the mean percentage of normal seasonal rainfall (see Table 1 for seasons) at Bikaner, Jodhpur and Khartoum (—) and at Atar, Nouakchott, Tessalit, Gao and Agades (---).



summer monsoon rainfall increased to a maximum of 16% above average from 1925 to 1927. Since the late 1920s, winter-spring rainfall has been increasing gradually and summer monsoon rainfall has been decreasing gradually, apart from temporary reversals in the 1950s.

The rather dramatic changes in the 1960s can thus be interpreted in a broader context as a continuation of trends first apparent in the late 1920s. At present, winter-spring rainfall is at a high point of an irregular fluctuation, superimposed on a longer term upward trend. Monsoon rainfall is at a low point of an irregular fluctuation, superimposed on a longer term downward trend. The present drought, extending from Mauritania to India, is a result of the cumulative effects of a decade of below average monsoon rainfall, with less and less rain each year.

Seasonal rainfall at a single station which receives both winter-spring rainfall, in association with troughs in the westerlies, and summer monsoon rainfall can be used as an indicator of rainfall trends throughout both zones; for example, compare the trends at Bikaner (Fig. 4) with

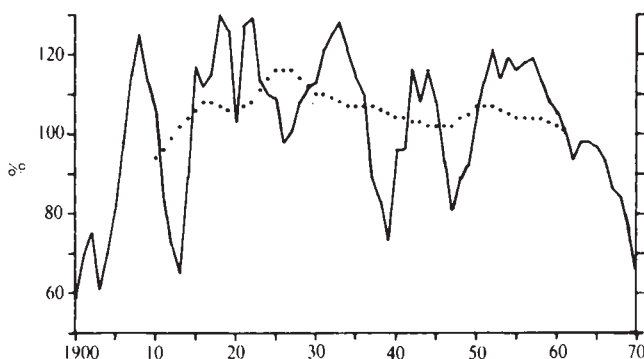


Fig. 2 —, Five-year running means of the mean percentage of normal summer rainfall at Atar, Nouakchott, Tessalit, Gao, Agades, Khartoum, Bikaner and Jodhpur; (.), 20-yr running means. (See Table 1 for period of records and seasons.)

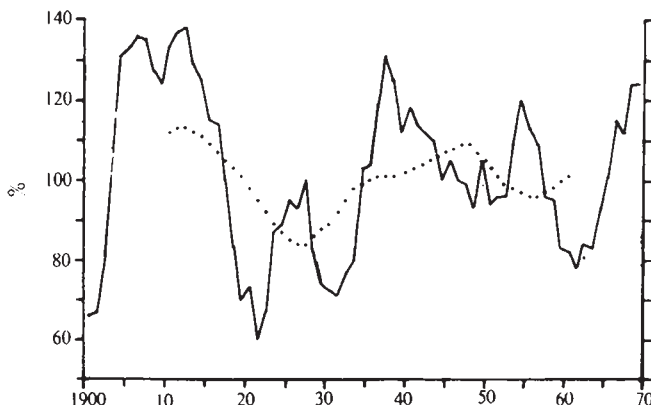


Fig. 3 —, Five-year running means of the mean percentage of normal winter-spring rainfall at Atar, Marrakech, Laghouat, Tunis, Tripoli, Jerusalem, Beirut, Mosul, Shiraz, Bikaner, Jodhpur, Muscat, Jiddah and Riyadh; (.), 20-yr running means. (See Table 1 for period of records and seasons.)

those for the winter-spring and summer rainfall belts as a whole (Figs 2 and 3). The winter-spring rainfall at Bikaner (mean 29 mm) is lower and relatively more variable than the summer monsoon rainfall (mean 266 mm).

## Rainfall and Changes in the General Circulation

Figure 5 shows the frequency of the westerly weather type over the British Isles since 1900 (ref. 2). This weather type indicates highly mobile weather systems, giving mild, wet winters over England. It is linked to a strong zonal westerly circulation in the mid-tropospheric circumpolar westerlies<sup>2</sup>. It can be deduced that the strength of the westerly circulation reached a peak in the mid-1920s, and that since then it has been decreasing (Fig. 5). There is much other evidence to indicate that the strength of the zonal circulation reached a peak in about 1930 in

**Table 1** Seasonal Rainfall at Eight Stations towards the Northern Limit of Summer Monsoon Rainfall in North Africa and India, and at Fourteen Stations with Winter-Spring Rainfall in Africa to the North of the Sahara and in the Middle East

| Country      | Station    | Period    | Season         | Mean (mm) | Range (mm) |
|--------------|------------|-----------|----------------|-----------|------------|
| Mauritania   | Nouakchott | 1935-36   | June-October   | 126       | 15-259     |
|              |            | 1941-72   |                |           |            |
|              | Atar       | 1933-36   | July-October   | 79        | 4-220      |
|              |            | 1941-72   |                |           |            |
| Mali         | Tessalit   | 1951-72   | May-September  | 89        | 29-178     |
|              |            |           |                |           |            |
|              | Gao        | 1932-36   | May-October    | 245       | 88-429     |
|              |            | 1941-72   |                |           |            |
| Niger        | Agades     | 1930-36   | May-September  | 142       | 37-285     |
|              |            | 1941-72   |                |           |            |
| Sudan        | Khartoum   | 1903-72   | June-September | 156       | 26-382     |
| India        | Bikaner    | 1898-1972 | May-October    | 266       | 42-753     |
|              | Jodhpur    | 1898-1972 | May-October    | 339       | 37-1,140   |
| Mauritania   | Atar       | 1933-36   | November-April | 24        | 0-75       |
|              |            | 1941-72   |                |           |            |
| Morocco      | Marrakech  | 1917-72   | January-May    | 143       | 45-358     |
| Algeria      | Laghouat   | 1950-70   | January-May    | 79        | 22-133     |
| Tunisia      | Tunis      | 1920-72   | January-May    | 217       | 102-389    |
| Libya        | Tripoli    | 1902-72   | January-May    | 153       | 16-383     |
| Israel       | Jerusalem  | 1898-1972 | October-May    | 601       | 202-1,125  |
| Lebanon      | Beirut     | 1898-1972 | October-May    | 893       | 393-1,600  |
| Iraq         | Mosul      | 1923-70   | November-May   | 374       | 188-565    |
| Iran         | Shiraz     | 1948-71   | November-May   | 303       | 73-581     |
| India        | Bikaner    | 1898-1972 | November-April | 29        | 0-104      |
|              |            |           |                |           |            |
|              | Jodhpur    | 1898-1972 | November-April | 21        | 0-90       |
|              |            |           |                |           |            |
| Oman         | Muscat     | 1902-30   | October-May    | 96        | 0-340      |
|              |            | 1935-42   |                |           |            |
| Saudi Arabia | Jiddah     | 1951-59   |                |           |            |
|              |            | 1951-72   | November-May   | 65        | 0-201      |
|              |            | 1941-44   | November-May   | 94        | 0-230      |
|              | Riyadh     | 1951-72   |                |           |            |

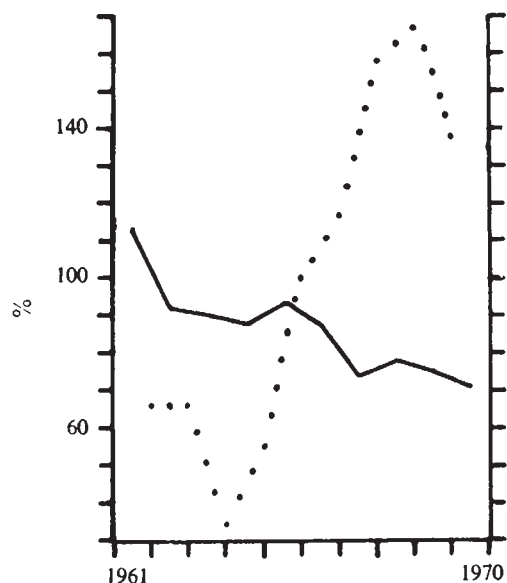


Fig. 4 Five-year running means of summer (—) and winter-spring (....) rainfall at Bikaner (Rajasthan, India) as percentages of the normal seasonal rainfall (see Table 1).

the northern hemisphere and in about 1900–10 in the southern hemisphere, and that it has since been decreasing<sup>2,3</sup>.

The fluctuations in the strength of the zonal circulation (Fig. 5a) are almost exactly paralleled by fluctuations of monsoon rainfall (Fig. 5c) and opposed by fluctuations of winter-spring rainfall over the Mediterranean and the Middle East (Fig. 5b). Rainfall trends in England have also closely paralleled the fluctuations in the strength of the zonal circulation, reaching a peak about 1930 and subsequently decreasing<sup>4</sup>. Thus, by also regarding Fig. 5a as representing rainfall trends in England, Fig. 5a–c summarizes the latitudinal fluctuations of rainfall this century in relation to changes in the strength of the zonal circulation.

There is considerable evidence of longer term fluctuations in the strength of the zonal circulation, and also of rainfall over England<sup>2,3</sup>. The zonal circulation and rainfall in England and Wales (Fig. 6) both attained peaks in the 13th century and declined to minima about 1700, rising again to the peak in about 1930. From the close relationship between the strength of the zonal circulation and rainfall over England, and the equally close relationship between the strength of the zonal circulation and monsoon rainfall Figs 5a and c), it is reasonable to assume that the long term trends of summer rainfall in the Sahel and in north-west India have closely paralleled the rainfall trends in England (Fig. 6), although the magnitude of the fluctuations has been greater. Conversely, trends of winter-spring rainfall over the Mediterranean and the Middle East have probably opposed the rainfall trends in England.

The high degree of correlation in the rainfall trends at different stations within the rainfall belts, and also between the belts, suggests that there are mechanisms controlling rainfall which operate on an intercontinental scale. The causal relationships are to be found in the sympathetic responses of both the large scale horizontal eddies in the circumpolar vortex and the meridional circulation in the tropics to change in the strength of the general circulation.

With a strong zonal circulation (Fig. 7a) relatively long, small amplitude waves in the circumpolar westerlies tend to be restricted to mid-latitudes; that is, the circumpolar vortex is contracted. (The dynamics of the relationship between the wave characteristics in the circumpolar westerlies, the zonal wind speed, and the change of the Coriolis parameter with latitude were explained by Rossby<sup>5,6</sup> in terms of the preservation of constant absolute vorticity

by windstream paths). Highly mobile depressions and ridges travel eastwards across the Atlantic to give changeable weather and high rainfall over England. Troughs in the circumpolar westerlies do not extend very far south into subtropical latitudes, with the consequence that rainfall over the Mediterranean and the Middle East is relatively low<sup>1</sup>. In the tropics the meridional circulation is strong and extends well to the north, as do the monsoon rains. A general warming occurs throughout the northern hemisphere<sup>2,7</sup>. Thus, when the zonal circulation is increasing in strength, monsoon rainfall in the Sahel and in north-west India increases, rainfall over the Mediterranean and the Middle East decreases, and rainfall over England increases. There is a northward shift of the principal climatic zones. There seem to have been such trends for several hundred years before 1200, and from about 1700 to 1930 (refs 2 and 3).

With a relatively weak zonal circulation (Fig. 7b), as at present, there are relatively short, large amplitude waves in the circumpolar westerlies and the strongest westerly flow tends to be shifted southwards; that is, the circumpolar vortex is expanded. Weather systems in mid-latitudes are slower moving and persistent anticyclones develop, which "block" the eastward moving depressions and deflect them to the north and to the south. Rainfall and the frequency of westerly winds over Britain decrease. Westerly troughs extend into subtropical, and even tropical, latitudes and rainfall over the Mediterranean and the Middle East is higher<sup>1</sup>. This is climatological evidence of the well known synoptic relationship between "blocking" over western Europe and heavy rainfall over the Mediterranean. But as the circumpolar vortex expands, so the tropical meridional circulation contracts<sup>8</sup>, and monsoon rains do not extend as far north<sup>1</sup>. There is also a general cooling throughout the northern hemisphere<sup>2,7</sup>. Thus, when the zonal circulation is decreasing in strength, rainfall decreases over England, increases over the Mediterranean and the Middle East, and decreases in the Sahel and in north-west India. There is a southward shift of the principal climatic zones. These downward trends were evident from about 1300 to 1700, and have again become

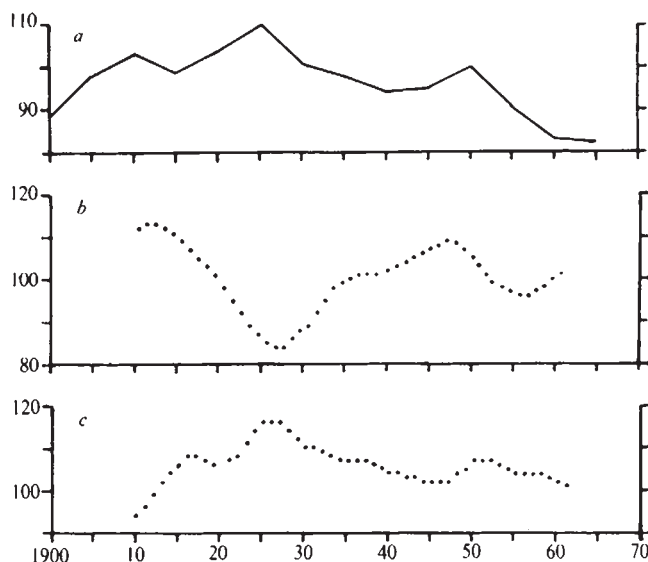


Fig. 5 a, Frequency ( $\text{d yr}^{-1}$ ) of the westerly weather type over the British Isles (from Lamb<sup>2</sup>). Ten-year mean values are plotted at 5-yr intervals against the middle of the decade to which they apply; b, 20-yr running means of the mean winter-spring rainfall at fourteen stations in north Africa and the Middle East (see Fig. 3 and Table 1); c, 20-yr running means of the mean summer monsoon rainfall at eight stations in the Sahel of north Africa and in north-west India (see Fig. 2 and Table 1).



evident since 1930 (refs 2 and 3); thus, rainfall in both England and the Sahel is at present at a low level but over the Mediterranean and the Middle East it is at a high level.

These interrelationships emphasize the unity of the general circulation and the importance that can be attached to the circumpolar vortex as a basis of reference, in terms of which practically all the reported changes in climatic elements fall into place<sup>2,9</sup>. Within an expanded or contracted circumpolar vortex the regional variations of seasonal rainfall are determined by the preferred longitudinal positions of the troughs and ridges.

Although the correlation between winter-spring and summer monsoon rainfall in individual years is statistically highly significant, it accounts for only about a third of the variance. There are periods when the winter-spring and summer monsoon rains have been positively correlated, for example, from 1905 to 1910, in the late 1940s, and in the late 1950s and early 1960s (Figs 2 and 3). In these years the circulation in the southern hemisphere may have exerted a strong influence on the northern hemisphere circulation, disrupting the more normal patterns. On a climatological time scale, the high correlation between the 20-yr running mean values of winter-spring and summer monsoon rainfall (Figs 5b and c) accounts for much more of the variance.

### Southward Shift of the Sahara

There is a very strong zonal arrangement of the isohyets to the south of the Sahara Desert. The mean distance between the 100 and 300 mm isohyets in the Sahel is about 280 km, which gives an average north-south gradient of annual rainfall of 1 mm per 1.4 km. The mean 5-yr running mean seasonal rainfall at Atar, Nouakchott, Gao, Tessalit, Agades and Khartoum centred on 1960 was 165 mm; in 1970 it was 100 mm. Thus, from 1960 to 1970 there was an average decrease of 65 mm in the mean seasonal rainfall at these stations in the Sahel zone. It can thus be estimated that since 1960 there has been a southward shift of the isohyets of  $65 \times 1.4$  km, which is 9.1 km, or approximately 9 km a year.

There was, however, a rapid and fairly continuous decrease in rainfall during the 1960s, which is not representative of longer term trends. From the 20-yr running mean values (Fig. 2), monsoon rainfall at these stations decreased by 15% of average from 1926 to 1961, that is, by an average of about 27 mm. Thus, in the 36 yr from 1926 to 1961 the isohyets shifted southwards by  $27 \times 1.4$  km, which is 37.8 km, or approximately 1 km per year. It is the desert climate, and in particular the isohyets, that have shifted southwards. There has probably also been an increase in the frequency of north-easterly winds transporting dust and sand from the Sahara to the Sahel. The southward shift of the Sahara Desert is an effect of these climatic changes, and not a cause.

Although the isohyets to the north of the Sahara do not show such simple zonality, it may be assumed, in view of the 17% increase in winter-spring rainfall since the late 1920s (Fig. 5b), that the Sahara has been retreating southwards at a similar rate.

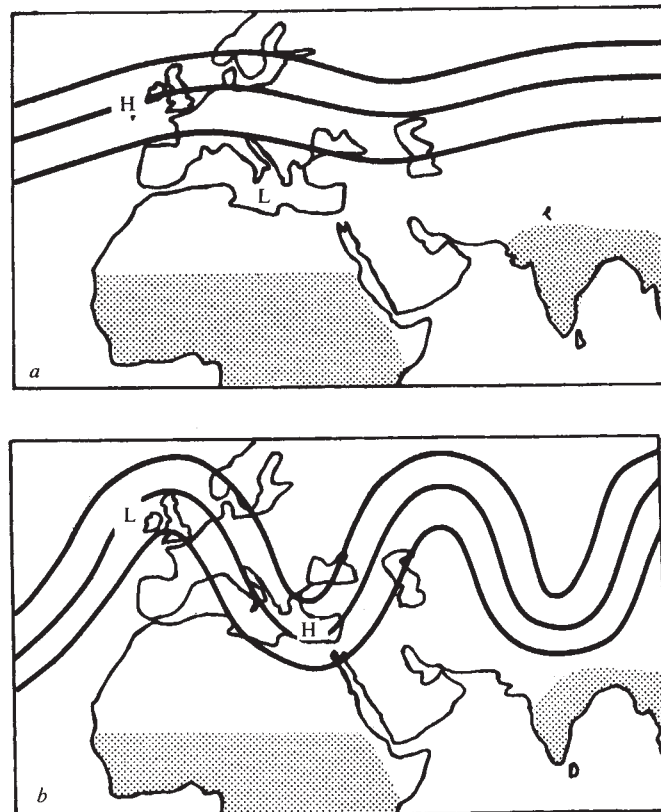


Fig. 7 Diagrammatic representation of extreme circulation types and associated rainfall patterns in the northern hemisphere. *a*, Strong zonal circulation giving high rainfall over England and allowing monsoon rains to extend well to the north in Africa and India; *b*, weak zonal circulation giving low rainfall over England and suppressing the northward extent of the monsoon rains. H, High rainfall; L, low rainfall.

Thus, any claims by man of success in controlling, or even reversing, the northward shift of the Sahara in north-west Africa should be assessed in the context of these natural changes. Similarly, building a line of trees across Africa to the south of the Sahara, as recently suggested, would have an absolutely minimal effect in halting the southward spread of the desert climate.

There is a causal check against the southward shift of the climatic zones. From the hemispherical chart of the mean annual topography of the 500 mbar surface of constant pressure<sup>10</sup>, the average gradient of the altitude of the 500 mbar surface between 40° and 80° N, from 180° W to 70° E, was calculated to be approximately 1 geopotential metre (g.p.m.) per 10 km. From 1960 to 1970 there was a decrease of 12 g.p.m. in the annual mean altitude of the 500 mbar surface along 40° N, from 110° W to 70° E (ref. 1). This signifies that the circumpolar vortex expanded by some 120 km, which, in view of the crude estimates, is very close to the figure of 91 km for the southward shift of the isohyets in the Sahel.

### Long Term Rainfall Prospects

At present there is no uniformly accepted theory of the general circulation of the atmosphere to include the fluctuations on these different time scales; therefore, physically based climatological rainfall forecasts cannot be produced. It is thus important to monitor the climatic changes, to delineate the areas over which the trends are similar, and to relate the changes to parameters of the general circulation. Theoretical models of the general circulation can then be checked against empirical models. Until we do have a generally accepted theory of the general circulation and until we can predict the variables that cause climate to

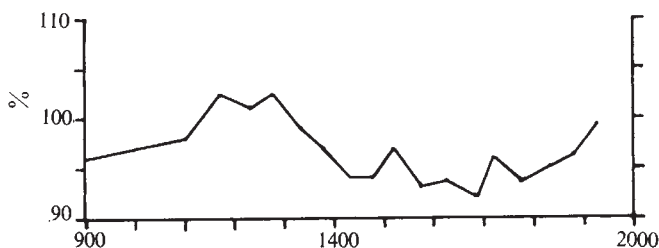


Fig. 6 Fifty-year averages of rainfall amounts (as percentages of the 1916-50 averages) over England and Wales (from Lamb<sup>2</sup>).

change, our best hope for climatological forecasting lies in the extrapolation of trends, and in determining what climatic changes accompany and follow the establishment of different circulation anomalies. Fortunately, there is a strong tendency for anomalous circulations to persist, not only from month to month, but also from year to year; for example, the strength of the general circulation has been decreasing and the circumpolar vortex has been expanding for the last 14 yr, and these trends have been evident in every month of the year.

Climate does change and it would be of great benefit to long term economic planning to know the future climatic changes. The choice lies between basing long term development either on climatic "normals" or on projected trends. Climatic "normals" are generally based on the periods 1920-50 and 1930-60, which were, in fact, highly abnormal. The alternative is to extrapolate trends observed over a long period of time and over large geographical areas.

After the five previous low points of monsoon rainfall this century, the rainfall has always recovered (Fig. 2). Conversely, after the four previous peaks of winter-spring rainfall, the rainfall has always decreased (Fig. 3). It is not possible to say whether we have yet reached the extreme of the present relatively short period fluctuation. From the available evidence, however, it seems likely that monsoon rainfall will increase and winter-spring rainfall will decrease towards 1980.

The main problem is to try to assess whether the general downward trend in the strength of the zonal circulation since the late 1920s marks the beginning of a long term downward trend, or whether it is just another "wobble". Statistical tests on the 50-yr average rainfall (Fig. 6) and temperature figures for England since 1100 afford some verification of quasi-periodicities around 100 and 200 years, especially the latter<sup>2</sup>. An approximate 200-yr period is also prominent in the calculated amount of radioactive carbon (<sup>14</sup>C) in the Earth's atmosphere<sup>11</sup>. These 200-yr rainfall variations are superimposed on an apparently larger amplitude wave of the order of 700 or 800 yr, only one cycle of which is covered.

The strength of the general circulation and monsoon rainfall have now been decreasing for almost 50 yr. Assuming a symmetrical 200-yr cycle, this downward trend will continue for a further 50 yr. Assuming that rainfall in the Sahel and Rajasthan continues to decrease at the same rate as it has since the late 1920s, by the year 2030 rainfall will be some 40-45% lower than in the optimum years. The isohyets, and thus the Sahara and Rajasthan Deserts, will have advanced some 100 km further south. There will, presumably, still be irregular fluctuations superimposed on this downward trend. At a low point on one of these fluctuations around the year 2030, the isohyets and the desert climate will be some 150 to 180 km further south than in 1930. If 1930 marked the peak of a 700-yr "cycle", and we are now on the long downward trend again, then these figures for the southward shift of the desert climate could well be underestimates.

To the north of the Sahara it is likely that the winter-spring rainfall will continue to increase, and that the northern fringe of the Sahara will retreat southwards.

## Significance for Long Term Development in the Sahel

Rainfall in England in the past 1,000 yr has changed by no more than about 10 or 11% (Fig. 6). Pittock<sup>12</sup> has estimated that regional precipitation changes of from 10 to 20% accompany a latitudinal shift of one or two degrees in the mean position of the centres of action. Because of the low mean annual rainfall (80 to 250 mm) and the strong north-south rainfall gradient in the Sahel, however, a southward shift of the climatic zones of only 1° of latitude in

100 yr means a decrease in rainfall of some 40-50%. In the more humid zone further south, where mean annual rainfall is up to 3,000 mm, the same 1° shift of the isohyets corresponds to a decrease in annual rainfall of only about 10%. There are cries of drought in England this summer, but what would be the consequences of a decrease in the mean annual rainfall from about 750 to 450 mm?

In the marginal areas along the southern fringes of the Sahara the human response to the low and highly variable rainfall (Table 1) is nomadic pastoralism, which is a very functionally adapted form of life. That the nomads have been unable to cope with the present shortage of rainfall is indicative of the severity of the present drought situation.

Until more is known about the physical factors and processes at work in climatic change, it would be highly irresponsible to contemplate modifying climate on this scale, even if it were technically feasible. In view of the probable long term downward trend in monsoon rainfall in the Sahel, there would seem to be two possible solutions. Massive international aid could be injected continuously into the Sahel states to increase the water and the food supplies in the drought-affected areas; but this would destroy the nomadic way of life. The natural solution would be for the nomads to shift southwards with the rains; but this would raise serious political problems.

In trying to gain an understanding of the present disastrous situation in west Africa, and in contemplating the future, it is surely significant to note that much of the economic development and population expansion in the former French colonies, leading to national independence, took place during a period of abnormally good rainfall: in 27 of the 39 yr from 1920 to 1958, rainfall throughout this semi-arid zone as a whole was above the long period average (individual years are not shown in Fig. 2). The present drought situation and the probable long term trends now seriously threaten the economic and political viability of the Sahelian states of west Africa.

Changes in the general circulation of the atmosphere produce climatic changes in all latitudes, but the magnitude of, for example, rainfall fluctuations varies greatly from region to region. The significance of the same changes in the general circulation is therefore much greater in the Sahel than in England. Climatology has an important role to play in long term planning, particularly in the Third World.

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# Function and Purification of Gene 4 Protein of Phage T7

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**T7 mutant in gene 4 synthesizes only short DNA fragments complementary to the H strand. By an *in vitro* complementation system which starts from intact T7 DNA, gene 4 protein has been purified.**

SEMICONSERVATIVE T7 DNA replication depends on the phage DNA polymerase (product of gene 5)<sup>1,2</sup>, a polynucleotide ligase<sup>3</sup> and, presumably, an unwinding protein (F. W. Studier, personal communication). Evidence has been presented that the T7 RNA polymerase is involved in the initiation of deoxynucleotide polymerization (ref. 4 and R. K. and E. Krause, unpublished). In addition to these proteins, the products of the phage genes 2 and 4 are essential for phage DNA replication<sup>5-8</sup>, but the biochemical functions of these proteins are unknown. Experiments will be described here which lead to better understanding of this role of the gene 4 product.

It has been shown that T7 endonuclease I specified by gene 3 has a dual function in T7 development. With gene 6 exonuclease<sup>9</sup> it degrades host cell DNA and is engaged in T7 DNA maturation<sup>8</sup>. When T7 infected cells are opened, however, the endonuclease loses its specificity and also degrades T7 DNA. As this enzyme is not required for semiconservative DNA synthesis *in vivo*<sup>8</sup>, gene 3 amber mutant infected cells rather than T7 wild type infected cells will be used as controls in the experiments described here.

## *In vivo* Function of Gene 4

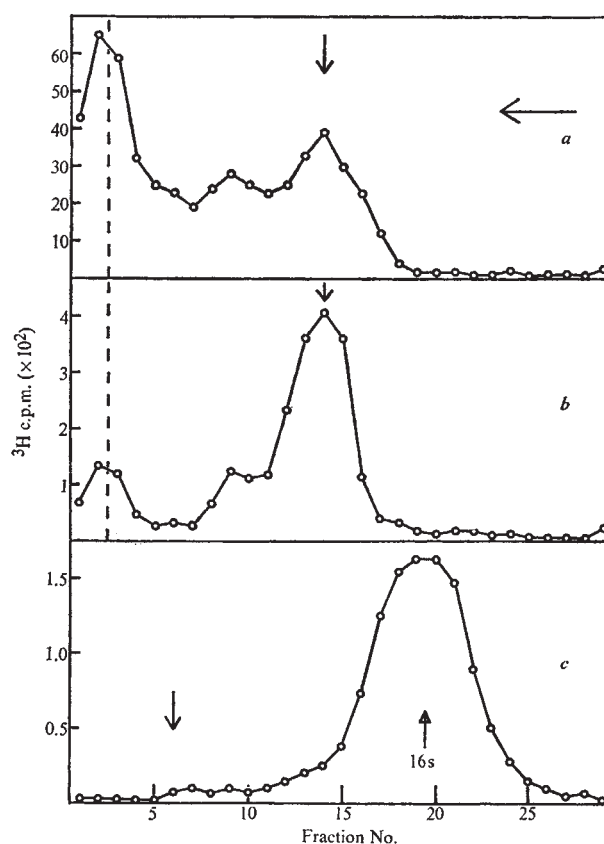
DNA synthesis in gene 4 infected cells is greatly reduced<sup>5-9</sup>. To obtain information on the product of this abortive DNA synthesis, cells infected with gene 3-gene 4 double amber mutant were pulse labelled with <sup>3</sup>H-thymidine. The extracted DNA was compared with <sup>3</sup>H-thymidine pulse labelled DNA from cells infected with gene 3 mutant by neutral sucrose gradient sedimentation (Fig. 1a and b).

The radioactivity incorporated in the absence of a functional gene 4 product sedimented with the rate of a T7 DNA marker (Fig. 1b). In the presence of the gene 4 product, however, most of the incorporated <sup>3</sup>H-thymidine sedimented faster than unit length T7 DNA<sup>8</sup> (Fig. 1a). After alkaline denaturation the DNA extracted from gene 3-gene 4 double mutant infected cells sedimented as a sharp peak at about 16S, that is, like DNA fragments of about 5,000 nucleotides<sup>10</sup> (Fig. 1c).

The pulse labelled DNA was hybridized with T7 H and L strands, respectively. Table 1 shows that the "gene 3-gene 4" DNA hybridized preferentially with the T7 H strand. The DNA from the control cells infected with gene 3 mutant hybridized equally well with H and L strand material.

## *In vitro* T7 DNA Replication

In all present systems for studying semiconservative DNA replication *in vitro*, DNA synthesis is interrupted in the living cell and continued *in vitro*. In interrupting *in vivo* replication the investigator must deal with the highly complex structure of the growth point. We therefore developed an *in vitro* system which starts from the intact double stranded DNA.



**Fig. 1** T7 DNA pulse labelled *in vivo*. *E. coli* H560 cells ( $F^-$ ,  $pol A^-$ ,  $endo I^-$ ,  $thy^-$ ,  $arg^-$ , amber suppressor-negative) were mitomycin treated<sup>10</sup> (to suppress host cell DNA synthesis) and infected either with a T7 gene 3 amber mutant (a) or a T7 double amber mutant in gene 3 and gene 4 (b)<sup>10</sup>. After 11 min at 30°C, <sup>3</sup>H-thymidine (10  $\mu$ Ci ml<sup>-1</sup>) was added for 1 min. DNA synthesis was stopped by 0.02 M KCN and the DNA extracted as described<sup>8</sup>. The DNA preparations were layered on linear sucrose gradients (20 to 5% in 1 M NaCl, 50 mM Tris-HCl, pH 7.5, 5 mM EDTA, on top of a CsCl shelf<sup>8</sup>) and centrifuged for 6 h at 38,000 r.p.m. in a Beckman SW 40 rotor. Fractions 13-16 from the gradient in (b) were recovered and used for DNA-DNA hybridization (Table 1) and velocity sedimentation in a linear 20 to 5% alkaline sucrose gradient (in 1 M NaCl, 5 mM EDTA, 0.1 M NaOH, pH 12.3) (c). Centrifugation was for 7 h at 38,000 r.p.m. in an SW 40 rotor. <sup>32</sup>P-T7 DNA served as sedimentation marker (vertical arrow). The horizontal arrow indicates the sedimentation direction.

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This system also proved useful for identification and purification of unknown proteins involved in initiation and elongation of T7 DNA replication. Gently lysed T7 infected *E. coli* cells which had been freed from the cellular envelope, ribosomes and about 99% of the endogenous cellular DNA and 95% of the endogenous T7 DNA<sup>4,11</sup> were concentrated. To these extracts intact double stranded T7 DNA was added as template. Protein preparations from cells infected with an amber mutant in gene 3 (3-extract) were incubated with T7 DNA with deoxynucleoside triphosphates in optimal conditions<sup>4</sup>. The *in vitro* synthesized DNA sedimented through neutral sucrose gradients in a pattern reminiscent of that from *in vivo* pulse labelled T7 DNA, except that a peak with a sedimentation coefficient of about 21S was observed (compare Fig. 1a with Fig. 2a). It could be shown that this additional peak is semiconservatively synthesized T7 DNA and is probably produced by fragmentation of replicating molecules. DNA strands of both polarities were synthesized (Table 1).

Protein preparations from cells infected with a gene 3-gene 4 double mutant ((3+4)-extract) synthesized mostly DNA which sedimented at the same rate as the T7 marker in neutral sucrose gradients (Fig. 2b). Only a smaller part of the incorporated radioactivity appeared in concatemers or fast sedimenting structures and the additional peak at 21S was never observed. After denaturation, the *in vitro* product sedimented at 16S in alkaline sucrose gradients. It also hybridized preferentially with the T7 H strand (Table 1).

In another experiment heavy T7 DNA in which thymine had been replaced by 5-bromouracil was incubated with (3+4)-extract. The 16S polynucleotide strands were purified by neutral and alkaline sucrose gradient sedimentation, as described above, and subjected to CsCl equilibrium centri-

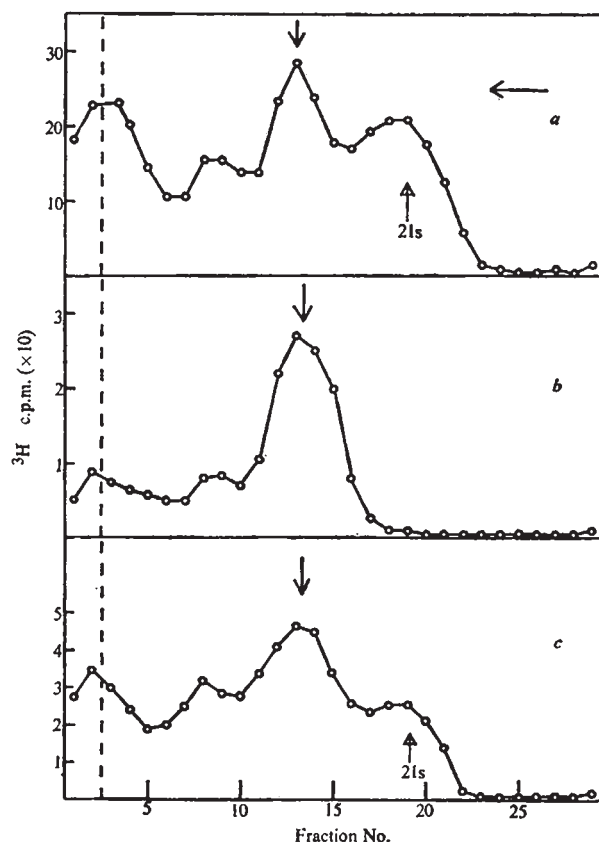


Fig. 2 T7 DNA synthesized *in vitro*. Protein preparation from cells infected with a T7 gene 3 mutant (a) and T7 double mutant in gene 3 and gene 4 (b and c), respectively, were prepared and incubated as described in Table 1. (c), 2  $\mu$ g isolated gene 4 protein (see Table 3) was added before incubation. The reaction products were characterized by velocity sedimentation in neutral sucrose (for details see Fig. 1).

Table 1 Strand Specificity

| Mode of DNA synthesis | Mutant or extract used                     | C.p.m. hybridized to H strand | L strand |
|-----------------------|--------------------------------------------|-------------------------------|----------|
| <i>In vivo</i>        | Gene 3 mutant                              | 8,120                         | 8,540    |
|                       | Gene (3+4) mutant                          | 4,580                         | 380      |
| <i>In vitro</i>       | 3 extract                                  | 5,690                         | 6,170    |
|                       | (3+4) extract                              | 820                           | 160      |
|                       | (3+4) extract plus (3+5) extract           | 2,960                         | 3,150    |
|                       | (3+4) extract plus isolated gene 4 protein | 1,430                         | 1,540    |

The strands of unlabelled T7 DNA were separated according to Summers and Szybalski<sup>12</sup>. T7 DNA L or H strands (5  $\mu$ g) were loaded on 'Millipore' filters<sup>13</sup>. DNA-DNA hybridization was carried out in  $1.25 \times \text{SSC}$ <sup>14</sup>. In all experiments the yield of hybridization was between 25 and 35%. Source of labelled DNA: (1) "*In vivo*": pulse labelled DNA from gene 3 mutant infected cells<sup>6</sup> was extracted with phenol and dialysed against  $1.25 \times \text{SSC}$ . Fractions 13-16 from the sucrose gradient of Fig. 1b (gene 3-gene 4 mutant infected cells) were dialysed against  $1.25 \times \text{SSC}$ . (2) "*In vitro*": Different H560 cultures grown in PB up to  $4 \times 10^8$  cells  $\text{ml}^{-1}$  were infected at a multiplicity of 10 with the indicated T7 amber mutants. After 13 min at  $30^\circ \text{C}$ , the cells were rapidly cooled, pelleted and resuspended in 10% sucrose, 50 mM Tris-HCl, pH 7.5, 10 mM  $\beta$ -mercaptoethanol, 0.1 M KCl, to give a cell density of  $4-8 \times 10^{10} \text{ ml}^{-1}$ . The cells were opened by lysozyme (final concentration 200  $\mu\text{g ml}^{-1}$ , 1 h at  $4^\circ \text{C}$  followed by 1 min at  $37^\circ \text{C}$ ). The lysate was centrifuged for 1 h at 100,000g. The supernatant (20  $\mu\text{l}$ ) was added to 0.2 ml of the incubation mixture composed of 50 mM Tris-HCl, pH 7.5, 100 mM KCl, 7 mM  $\text{MgCl}_2$ , 3 mM spermidine, 10 mM  $\beta$ -mercaptoethanol, 1  $\mu\text{g}$  native T7 DNA, 1 mM each of UTP, GTP, CTP and ATP, 0.05 mM each of dATP, dCTP and dGTP and 5  $\mu\text{M}$  of  $^3\text{H}$ -dTTP (specific activity 2 Ci  $\text{mmol}^{-1}$ ). After incubation for 30 min at  $37^\circ \text{C}$  sarcosyl and EDTA were added to final concentrations of 0.5% and 20 mM, respectively. The synthesized DNA was further purified by neutral sucrose gradient sedimentation before it was dialysed against  $1.25 \times \text{SSC}$  and used for hybridization.

Table 2 *In vitro* Complementation

|                                            | Deoxyribonucleotide incorporated (pmol) |
|--------------------------------------------|-----------------------------------------|
| (3+4) extract                              | 12                                      |
| (3+5) extract                              | 0.8                                     |
| (3+4) extract plus (3+5) extract           | 36                                      |
| (3+4) extract plus isolated gene 4-protein | 40                                      |
| 3 extract                                  | 94                                      |

For details see text and legends to Tables 1 and 3.

fugation. All the recovered radioactivity banded at the density of light T7 DNA (data not shown). This shows that the polynucleotide chains synthesized by (3+4)-extract are hydrogen bonded to the parental H strand.

We also asked how many fragments were synthesized along the parental H strand in the *in vitro* system. T7 DNA was incubated with (3+4)-extract in a ratio of one intact T7 DNA molecule to the amount of protein preparation equivalent to two infected cells. From the specific activity of the supplied  $^3\text{H}$ -dTTP, the amount of its incorporation into L fragments, and the length of the fragments, it can be estimated that an average of 1 to 1.5 fragments per parental T7 DNA molecule has been synthesized.

### *In vitro* Complementation

In Table 2, the incorporating activities of extracts from cells infected with various T7 amber mutants are compared. Less than 2% of the control synthesis was observed in extract from gene 3-gene 5-double mutant infected cells ((3+5)-extract). About 15% of the control synthesis was observed in (3+4)-extract. When equal amounts of both extracts were mixed before incubation a two to five-fold stimulation of DNA synthesis could be observed. The DNA synthesized in



**Table 3** Purification of Gene 4 Complementing Activity

| Fraction | Volume (ml) | Protein (mg ml <sup>-1</sup> ) | Activity (units ml <sup>-1</sup> ) | Specific activity (units mg <sup>-1</sup> ) | Recovered activity (%) |
|----------|-------------|--------------------------------|------------------------------------|---------------------------------------------|------------------------|
| I        | 30          | 21                             | 46                                 | 2.2                                         | 100                    |
| II       | 20          | 1.4                            | 53                                 | 38                                          | 82                     |
| III      | 7.0         | 0.58                           | 102                                | 176                                         | 55                     |
| IV       | 8.3         | 0.34                           | 69                                 | 203                                         | 44                     |
| V        | 4.0         | 0.11                           | 78                                 | 709                                         | 24                     |

A unit is defined as the amount of activity sufficient to stimulate 3-fold deoxynucleotide incorporation in the standard assay with 20  $\mu$ l of (3+4) extract which corresponds to about 20–40 pmol. The activities reported here were determined by assay of dilutions of each fraction with the same deficient extract. Protein was determined by Lowry *et al.*<sup>15</sup>

For purification of gene 4 protein, 30 ml (3+5) extract (fraction I) were loaded on a DEAE-cellulose column (9.0  $\times$  2.0 cm) which had been equilibrated with 50 mM Tris-HCl, pH 7.5, 10 mM  $\beta$ -mercaptoethanol (buffer A). A linear gradient of NaCl (0.05–0.6 M) was applied to the column. Gene 4 protein was eluted at a sodium chloride concentration of about 0.25 M (fraction II). After dialysis against buffer A fraction II was loaded on a phosphocellulose column (8.0  $\times$  0.9 cm) equilibrated with buffer A and eluted with a gradient from 0.05 to 0.8 M NaCl in buffer A. To the fractions containing gene 4 protein activity (fraction III) solid ammonium sulphate was added to 65% saturation. After 30 min at 0° C the precipitate was pelleted, dissolved in 0.5 ml buffer A and dialysed for 3 h against buffer A. Fraction III was placed on a 'Sephadex' G200 column (38  $\times$  2 cm) and eluted with buffer A. The fractions with gene 4 protein activity (fraction IV) were dialysed against 20 mM sodium phosphate buffer, pH 7.3, 10 mM  $\beta$ -mercaptoethanol (buffer B) and pumped on a 5.0  $\times$  0.9 cm hydroxyapatite column. The column was rinsed with buffer B and eluted by a 20–300 mM phosphate gradient. Gene 4 protein came off the column at about 100 mM phosphate to yield fraction V.

the mixed extracts sedimented like the DNA produced in the control 3-extract (compare Fig. 2a). In addition, the co-operative action of both extracts resulted in H and L strand synthesis with nearly equal efficiency.

### Isolation of Gene 4 Protein

By use of the complementation response as an assay, gene 4 protein was purified from (3+5)-extract. The recovery and

purity at each step are listed in Table 3. Fraction V complemented successfully (3+4)-extract, as was concluded from (1) a three to five-fold increase of the overall DNA synthesis (Table 2); (2) the synthesis of DNA which hybridized equally well with T7 H strand and L strand (Table 1), and (3) stimulation of the synthesis of concatemers and fast sedimenting structures and induction of 21S DNA synthesis (Fig. 2c).

The general function of gene 4 protein is to enable the replicative apparatus to synthesize DNA strands of H strand polarity. The biochemical reaction performed by this protein has still to be elucidated. A detectable DNA or RNA polymerizing activity was not associated with the isolated gene 4 protein, nor did the protein stimulate the enzymatic activity of isolated phage DNA or RNA polymerase.

All of the T7 mutants used in this work have been constructed by Dr F. William Studier. We thank Drs William E. Goodell and Heinz Schaller for comments on the manuscript.

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# LETTERS TO NATURE

## PHYSICAL SCIENCES

### Development of an H<sub>2</sub>O Atmosphere around Comet Kohoutek (1973f) and its Possible Detection

ALTHOUGH chemically stable "parent molecules" like H<sub>2</sub>O, CH<sub>4</sub>, NH<sub>3</sub> and C<sub>2</sub>N<sub>2</sub>, for the observed radicals in comets, have been suspected for more than thirty years<sup>1</sup> none of them has yet been observed spectroscopically. But the indirect evidence both for the existence, as well as perhaps the preponderance, of H<sub>2</sub>O in the nucleus has been growing steadily during the past ten years<sup>2–4</sup>. In particular the recent observations of two bright long period comets Tago-Sato-Kosaka (1969g) and Bennett (1969i) (as well as P/Encke) both in the vacuum ultraviolet and in the near violet with detectors on orbiters<sup>5–8</sup> and rockets<sup>9</sup> (which have shown extensive hydrogen and hydroxyl halos) as well as associated numerical models of the neutral atmospheres<sup>10–13</sup> strongly suggest H<sub>2</sub>O as the common parent of both these species.

In the circumstances a strong observational effort is in order for the spectroscopic detection of H<sub>2</sub>O in comets. The newly discovered great comet, Kohoutek (1973f), which is speeding towards perihelion passage at about the end of the year (see Fig. 1) and which bids fair to be the most spectacular comet of the century, may provide an excellent opportunity for directly observing a "parent molecule" (namely, H<sub>2</sub>O) for the first time.

Here we consider the continuous development of an H<sub>2</sub>O atmosphere around the nucleus as this comet approaches the Sun and discuss the possibility of its detection.

Complete hydrodynamic models of cometary atmospheres composed of H<sub>2</sub>O and its photodissociation fragments OH, H and O coupled through collisional interaction as well as production and loss processes have been developed both for a central source<sup>10</sup> and for a central source supplemented by a distributed halo of icy grains<sup>11</sup> (which has been suggested by the work of Delsemme and colleagues<sup>14–16</sup>). Applied specifically to the case of comet Bennett (1969i) they have been successful in explaining, rather well, the observed Ly $\alpha$  brightness profile<sup>10</sup> and the streaming velocity of hydrogen<sup>11</sup>. We have

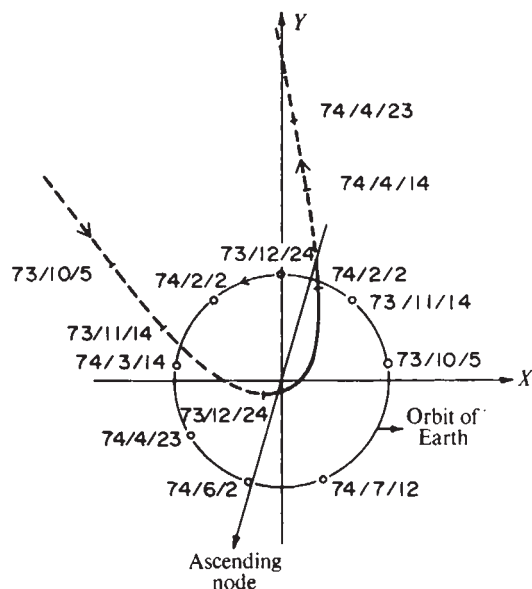


Fig. 1 The ecliptic projection of the orbit of comet Kohoutek (1973f). (Orbital elements:  $\omega=37^\circ.8$ ,  $\Omega=257.9$ ,  $i=14.3$ ,  $q=0.141$  AU,  $e=0.9999$ .) ---, Portions of the orbit below the ecliptic.

computed, on essentially the same lines, the density and velocity profiles for an ( $\text{H}_2\text{O}$ , OH, H, O) atmosphere of comet Kohoutek (1973f) at various heliocentric distances (see Fig. 2a) for the two extreme values ( $r_N=10$  km and  $r_N=100$  km) of the very uncertain nuclear radius. Here we present the density and velocity profile of only  $\text{H}_2\text{O}$  for the case  $r_N=10$  km (see Fig. 3a and b). The rate of production of  $\text{H}_2\text{O}$  at various heliocentric distances is obtained from the values calculated by Delsemme and Miller<sup>16</sup> for a rotating cometary nucleus with an albedo  $A=0.1$ . It is also assumed that the  $\text{H}_2\text{O}$  evaporating from the nucleus starts off slightly supersonically.

We note the steady contraction of the  $\text{H}_2\text{O}$  coma with decreasing distance for  $R \lesssim 2$  AU, following the slight increase between 2.5 AU and 2 AU. The  $\text{H}_2\text{O}$  velocity in the outermost regions is seen to increase steadily from about  $1.3 \text{ km s}^{-1}$  to about  $1.9 \text{ km s}^{-1}$  as  $R$  decreases from 2.5 AU to 0.5 AU.

The fairly recent detection of  $\text{H}_2\text{O}$  in interstellar space at  $1.35 \text{ cm}$  due to its  $6_{16} \rightarrow 5_{23}$  rotational transition<sup>17</sup> prompted Clark *et al.*<sup>18</sup> to carry out a radio search for  $\text{H}_2\text{O}$  in comet Bennett (1969i), soon after perihelion passage, by means of the same transition. An upper limit  $\sim 2 \times 10^{17}$  molecules  $\text{cm}^{-2}$  for the column density was derived from the negative result obtained. This upper limit, obtained by assuming that the temperature of  $\text{H}_2\text{O} \sim 200 \text{ K}$ , may in fact be rather low because, except within a few 100 km of the nucleus, the  $\text{H}_2\text{O}$  expands almost adiabatically and when  $r \gtrsim 1,000 \text{ km}$ ,  $T \lesssim 100 \text{ K}$ <sup>10,11</sup>. If this lower temperature is assumed the corresponding upper limit for  $\text{H}_2\text{O}$  column density is about  $10^{18}$  molecules  $\text{cm}^{-2}$ . The calculated  $\text{H}_2\text{O}$  column densities in the case of comet Kohoutek are given in Table 1. These values correspond to the case  $r_N=10 \text{ km}$ . If  $r_N=100 \text{ km}$  they would be, in every case, about an order of magnitude larger. Consequently the  $1.35 \text{ cm}$  line of  $\text{H}_2\text{O}$  may very well be detectable at least between December 1, 1973, and February 1, 1974, when  $R \lesssim 1 \text{ AU}$ .

High dispersion ( $5.6 \text{ \AA mm}^{-1}$ ) near-infrared spectra of Mars taken about 10 yr ago at the Coudé focus of the Mount Wilson 100-inch reflector showed several weak absorption lines of the rotation-vibration band of  $\text{H}_2\text{O}$  near  $0.82 \text{ }\mu\text{m}$ <sup>19,20</sup>. The Martian water lines were redshifted by  $\sim 0.42 \text{ \AA}$  to the wings of their telluric counterparts (half-width  $\sim 0.5 \text{ \AA}$ ) due to the relative velocity of the Earth and Mars at that time ( $\Delta V \approx +15 \text{ km s}^{-1}$ ). The Martian lines, which were strongest over the poles ( $\sim 3$  to  $5 \text{ m}\text{\AA}$ ), suggested a Martian polar abundance

of 5 to  $10 \text{ }\mu\text{m}$  of precipitable water. Subsequent higher dispersion ( $4 \text{ \AA mm}^{-1}$ ,  $2 \text{ \AA mm}^{-1}$ ) spectroscopy of Schorn *et al.*<sup>21,22</sup>, as well as that of Tull ( $1.9 \text{ \AA mm}^{-1}$ )<sup>23</sup>, has confirmed the earlier results by detecting clear Martian lines of the  $0.82 \text{ }\mu\text{m}$  water band blue shifted by  $\sim 0.3$  to  $0.5 \text{ \AA}$  to the wings of their telluric components. These authors have suggested values ranging from  $\sim 10 \text{ }\mu\text{m}$  to  $\sim 50 \text{ }\mu\text{m}$  for precipitable  $\text{H}_2\text{O}$ , which seems to vary with time as well as location on the planet.

Similar observations of Venus in the near infrared ( $\sim 0.8$  to  $3.0 \text{ }\mu\text{m}$ ) have shown the Venusian absorption in the  $0.82 \text{ }\mu\text{m}$  band as well as in other water bands (see Fink *et al.*<sup>24</sup> for a summary). While the estimates of the Venusian water vapour content vary by over three orders of magnitude, the smallest values<sup>24,25</sup> (obtained using aircraft-borne interferometers to observe the  $1.4$ ,  $1.9$  and  $2.7 \text{ }\mu\text{m}$  bands) are of the order of  $1 \text{ }\mu\text{m}$  precipitable  $\text{H}_2\text{O}$ .

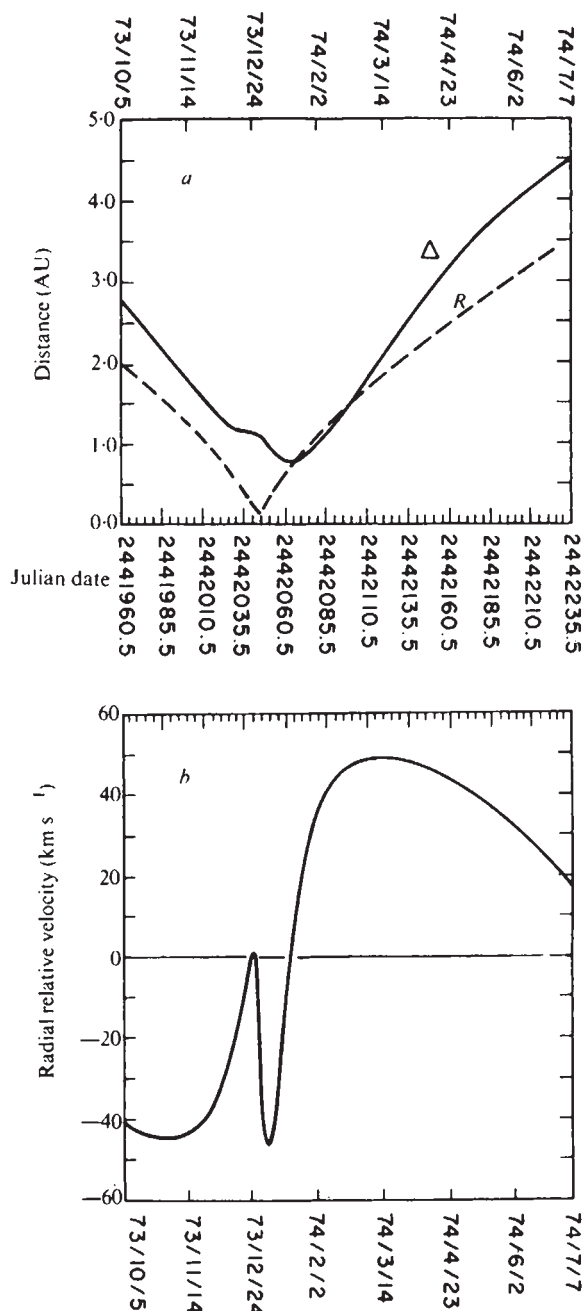


Fig. 2 a, Time variation of the heliocentric distance ( $R$ ) and the geocentric distance ( $\Delta$ ) of comet Kohoutek; b, time variation of the radial velocity of comet Kohoutek with respect to the Earth.



The water vapour content in the atmosphere of Kohoutek for various heliocentric distances is given in Table 1 for the case:  $r_N = 10$  km. If  $r_N \approx 100$  km each of these values is larger by an order of magnitude. The radial velocity of the comet with respect to the Earth is shown in Fig. 2b. It is seen that except for a period just before perihelion ( $0.2 \lesssim R \lesssim 0.5$  AU) this velocity is more than adequate—Doppler-shifting the lines of the  $0.82 \mu\text{m}$  band by 0.3 to  $1.2 \text{ \AA}$ , and those of the larger wavelength bands by proportionately larger amounts. Estimates of the time variation of the apparent magnitude of the comet are available<sup>25</sup>. It seems that the comet will remain fainter than about  $-1$  mag until  $R \lesssim 0.5$  AU (compare  $m_v$  of Mars at

also confined our discussion to the case of  $\text{H}_2\text{O}$ , not because we preclude the possible detection of other suggested "parent molecules" but because we expect that  $\text{H}_2\text{O}$ , by virtue of its expected preponderance over all other volatiles in long period comets<sup>26,27</sup>, is the likeliest candidate for detection.

In conclusion we hope that this communication will further stimulate a strong observational effort (from photographic to microwave frequencies) for the detection of  $\text{H}_2\text{O}$  in comet Kohoutek, using both ground-based instruments and instruments carried in high flying jet aircraft, balloons and rockets. The apparent lack of the appropriate equipment in Skylab is to be regretted.

**Table 1** Calculated Column Densities of  $\text{H}_2\text{O}$  for Comet Kohoutek

| $R$ (AU)                                                                       | 2.5                  | 2.0                  | 1.5                  | 1.0                  | 0.75                 | 0.5                  |
|--------------------------------------------------------------------------------|----------------------|----------------------|----------------------|----------------------|----------------------|----------------------|
| Column density (molecules $\text{cm}^{-2}$ )                                   | $1.1 \times 10^{17}$ | $4.5 \times 10^{17}$ | $1.4 \times 10^{18}$ | $2.2 \times 10^{18}$ | $2.9 \times 10^{18}$ | $3.3 \times 10^{18}$ |
| Vertical content of precipitable $\text{H}_2\text{O}$ vapour ( $\mu\text{m}$ ) | $3.3 \times 10^{-2}$ | $1.4 \times 10^{-1}$ | $5.2 \times 10^{-1}$ | $6.6 \times 10^{-1}$ | $8.7 \times 10^{-1}$ | $\sim 1$             |

opposition  $\approx -2$ ). All these factors seem to suggest that  $\text{H}_2\text{O}$  may well be detectable in this comet, by any of the methods described, when  $R \lesssim 0.75$  AU, particularly if  $r_N$  is closer to 100 km than to 10 km.

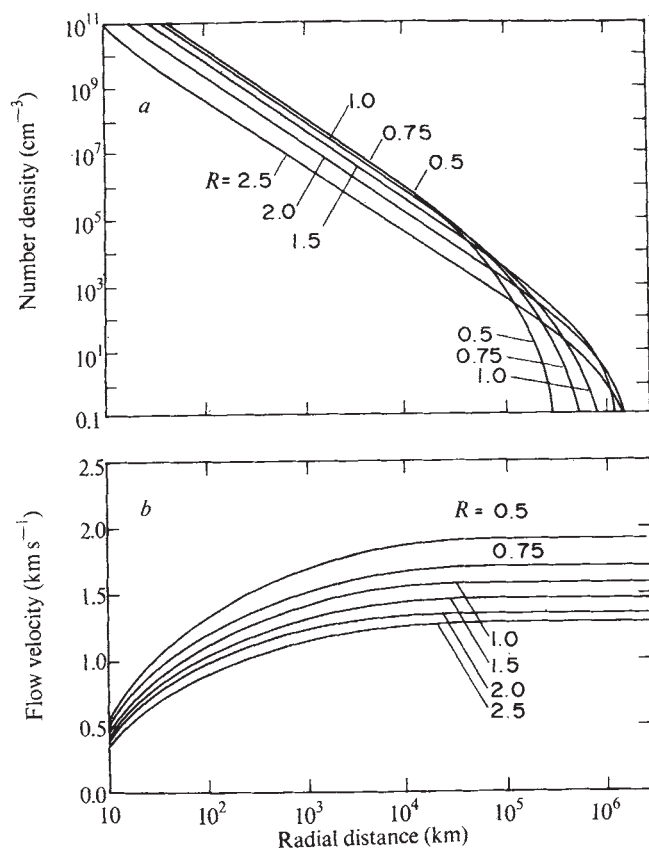
It has not been our intent, in this brief communication, to review all the possible ways in which  $\text{H}_2\text{O}$  may be detected in the atmosphere of comet Kohoutek, but rather to suggest that some of the methods used for its detection in other Solar System bodies, as well as a method used unsuccessfully for its search in a previous long period comet, may successfully be used for its determination in the case of Kohoutek. We have

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**Fig. 3** *a*,  $\text{H}_2\text{O}$  density profile in the atmosphere of comet Kohoutek at various heliocentric distances ( $R$ ); *b*,  $\text{H}_2\text{O}$  velocity profile in the atmosphere of comet Kohoutek at various heliocentric distances ( $R$ ).

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## Prospects for Comet Kohoutek (1973f)

MANY comets have developed in an extremely irregular and unexpected manner. Large changes of intrinsic brightness have occurred quite suddenly, and curious and rapid changes of shape associated with the coma have often been observed, sometimes with disruption and complete disappearance of the comet. The standard empirical law of brightness is

$$L = L_0 \Delta^{-2} r^{-n}$$

where  $\Delta$  and  $r$  denote distance from the Earth and Sun respectively; the index  $n$  ranges<sup>1</sup> from about  $-1.8$  to  $+11.4$ , with an average value of about  $3.3$ . Although the absolute brightness of most comets increases with diminishing solar distance, the negative values of  $n$  arise from a small number that have grown fainter as they approached the Sun.

Thus long term predictions of future cometary brightnesses are likely to be subject to considerable uncertainty. Although the designation already accorded to Kohoutek as "the comet of the century" may prove to have been justified, present estimates (August 19, 1973) of its eventual maximum total brightness as approaching  $-10$  mag with  $n=6$  may be optimistic. A figure of perhaps  $-4$  mag  $\pm 2$  mag might well stand more than a 50% chance of bracketing the measured value.

Since the minimum distance of Kohoutek from Earth will be at least 0.8 AU (ref. 2), there seems no possibility of determining whether or not the comet contains a solid icy nucleus, even of as large a diameter as the 30 km at present being suggested for it. But since the perihelion distance  $q$  will be as small as 0.14 AU, direct solar heating at that stage could raise material exposed to it, whatever the structure of the comet, to a temperature of about 700 K; even much further out, at 0.64 AU for example, it will raise it to a temperature of some 350 K, sufficient to boil the "ices" of many volatile compounds. Accordingly, on the basis of the icy nucleus model, activity in the form of tail production and interaction with the solar wind should be well under way early in December 1973, since a distance  $r=0.64$  AU is reached about December 11. Activity should continue until mid-January 1974 or later because of the asymmetry of tail production with regard to perihelion.

But on the basis of the widely spaced dust particle swarm model for the structure of comets<sup>3</sup>, more precise prediction can be made for the time of onset of strong tail production. This is substantially different from that suggested by the icy nucleus model, and may provide a crucial test between the two. On the dust cloud theory, tail production results from collisions between particles converging from the two sides of the median orbital plane defined by the motion of some central particle of the comet; each of the particles not moving exactly in this plane must automatically in its path round the Sun cross through the plane twice per revolution, once on the perihelion side at a node  $N$  between the ends  $L$  and  $L'$  of the latus rectum  $LSL'$  ( $S$  denoting the Sun), and once on the aphelion side at a node  $N'$ . On the latest available orbital data, Kohoutek will reach  $L$  at about December 23.5, 1973, perihelion  $A$  at December 28.5, and  $L'$  at January 3.5, 1974 (Fig. 1). The perihelion side of the orbit  $LAL'$  is described in just under 11.8 d, while the aphelion side  $L'A'L$  may take  $10^3$  or  $10^4$  yr or even far longer. The indications are that the present osculating period may be in excess of  $5 \times 10^4$  yr.

Thus on the aphelion side, the average number of particles passing through the median orbital plane in a given time interval will be much less than on the perihelion side. The numbers are in the ratio of 12 d to the effective period. Further, the relative velocity of these colliding particles is proportional to the orbital speed of the comet, and to the sum of the maximum distances from the median plane

that the colliding particles attain beforehand, both of which depend on the general overall size of the comet and the particle distribution within it. Since the velocity on the perihelion arc, which increases from about  $79 \text{ km s}^{-1}$  at  $L$  to about  $112 \text{ km s}^{-1}$  at perihelion, is much greater than on the portions of the aphelion side over which the comet will be observable, there would be only very slight tail production before December 23.5, 1973, even though the comet will by then be much closer to the Sun than Mercury. Solar heating may be sufficiently effective for very slight production much earlier of a gaseous tail that might be detectable by long photographic exposure; if operative, this heating process would apply on either hypothesis, though probably more effectively on small particles than on a solid nucleus because of the greater surface area of the former, mass for mass. But in addition, unless the nodes  $N'$  associated with the separate orbits of the particles were very specially distributed on the aphelion side of the orbit, where the comet spends almost all its life, the distribution of the nodes  $N$  will tend to concentrate very strongly round perihelion. This is because of the greatly elongated form of the orbit. Thus it may be predicted further that only very slight tail production, possibly none of a dust variety, will occur before about December 27, but thereafter activity will intensify to reach its maximum shortly after perihelion, continuing, as far as the production of a dust tail is concerned, until near January 3, 1974. Since a great deal of gaseous material could be released as a result of impact heating in the collisions, aided to some extent by solar radiation, a gaseous component to the comet and tail may persist beyond the point  $L'$  after January 3.5.

As with many comets of small  $q$ , Kohoutek is extremely awkwardly situated when near perihelion for observation from Earth because of the proximity of the Sun to the line of sight (Fig. 2). At perihelion, the comet will be little

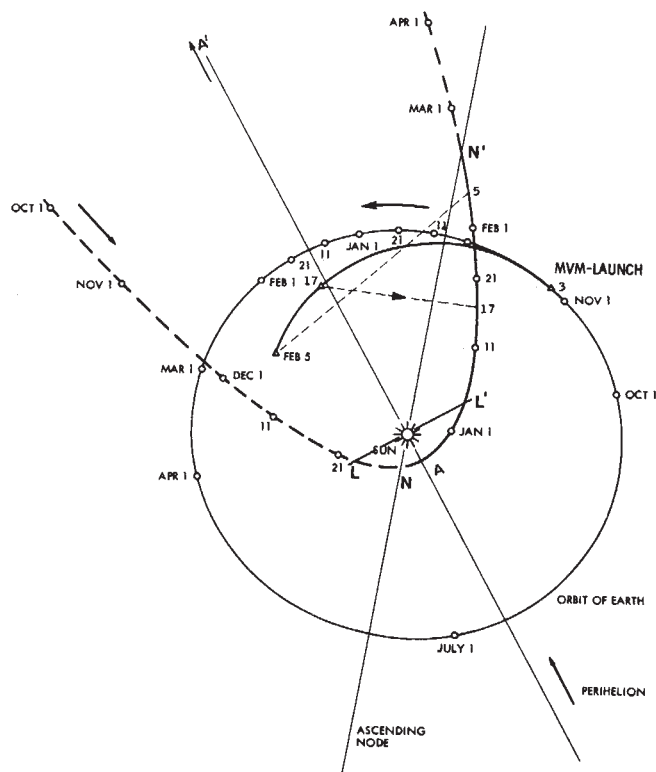


Fig. 1 Orbit of comet Kohoutek between October 1, 1973, and April 1, 1974. The line  $L$ -Sun- $L'$  is the latus rectum.  $T=$  December 28.7, 1973;  $\omega=37.3^\circ$ ;  $\Omega=257.9^\circ$ ;  $i=14.3^\circ$  (1950.0);  $q=0.141$  AU. MVM=Mariner Venus-Mercury probe.



more than  $2^\circ$  east from the centre of the Sun, and at the predicted time for the beginning of intense tail production it will be only about  $1^\circ$  to the west. Also the tail itself, if chiefly directed away from the Sun, will be on the far side of the Sun and comet, close to the Earth-comet line. Thus at the time of onset of strong tail production, the phenomenon will not be readily observable, if at all, from Earth. The Mariner 10 Venus-Mercury craft, scheduled to be in flight then, moves more or less with the rotating Sun-Earth line, and unfortunately it will be just as poorly situated for this purpose, even if it could use its cameras in a direction close to the Sun. In fact it is not proposed to turn the MVM instrumentation onto the comet until January 17, 1974, by which time tail production may have fallen to very weak intensity. Other space vehicles may be orbiting the Earth at the time and will be able to make successful observations at and near perihelion in spite of the closeness of the Sun. But if the tail should be flowing more or less directly away from the Sun at all stages, then by January 1, 1974, this general direction would make an angle of some  $60^\circ$  to the Earth-comet line, with the comet about  $10^\circ$  from the Sun, while by January 11, if a tail still persists, the angle would be just under  $90^\circ$  and the comet just over  $30^\circ$  from the Sun. Thus observation of the tail shortly after perihelion should become possible from Earth, and the geometrical conditions continue suitable as the comet recedes from the Sun.

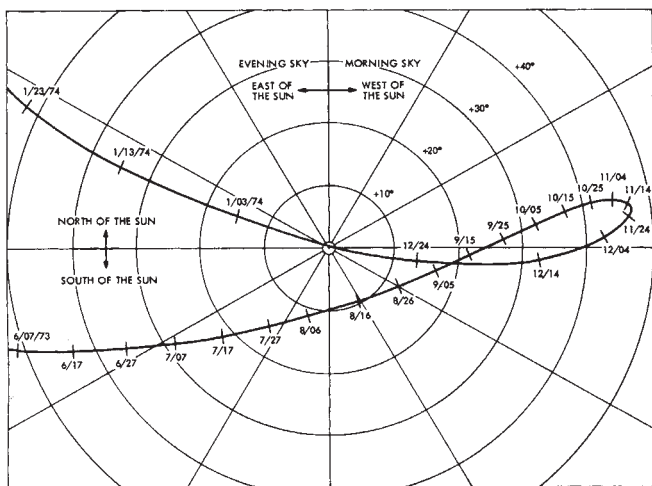


Fig. 2 Trajectory of comet Kohoutek (1973f) with respect to the Sun as seen from Earth (markers at 10 d intervals).

As for most comets that show emission spectra, Kohoutek should certainly exhibit the presence of volatile radicals, but, in view of the fairly high speed at perihelion, heating of particles locally through collisions may be sufficiently great for there to be shown not only the presence of sodium, but even such elements as magnesium, nickel, and possibly iron, already claimed to have been detected in other comets of small  $q$ . Furthermore, if observations from outside the terrestrial atmosphere are made, many substances with strong lines in the ultraviolet may well be detected.

It is a property of a group of particles all moving in much the same orbit that the extent of the distribution in the direction of the orbital motion will change in direct proportion to the speed. This is easily seen by considering just two particles describing the same orbit with one slightly ahead of the other. When Kohoutek reaches 1 AU from the Sun at about November 26, 1973, the tangent to its path will be at more than  $60^\circ$  to the Earth-comet line, and foreshortening of the dimension along the orbit will thus be only a factor of 0.9. The speed at this stage

will be about  $42 \text{ km s}^{-1}$ . At L (December 23.5), the angle is about  $90^\circ$  and the speed  $79 \text{ km s}^{-1}$ , so the actual extent along the orbit would have about doubled, with the comet then about  $11^\circ$  from the Sun. At perihelion the extent would be tripled, but the comet will be directly beyond the Sun. It would accordingly be of interest to make a systematic series of measurements of the dimensions of the coma in relation to the orbit to determine whether there are signs of this effect. But it does not follow that the dimensions of the comet as observed will necessarily also vary in just this way, for it will only be the detectable extent of the comet that can be measured, and the boundary of this may not always correspond to the same particles. If a swarm of particles expands indefinitely, at some stage the observed size must start to decrease and the swarm eventually become invisible because of its diffuseness.

The extent of the comet perpendicular to the general orbital plane may also change on the dust-swarm hypothesis. With the nodes N clustered round perihelion, the comet may well exhibit contraction of its coma in the transverse direction on its inward path, and expansion as it recedes. With the inclination of the cometary orbit only  $14^\circ$ , the Earth will be well situated for such measures from late August 1973 till about December 24, during which interval the distance from the Sun increases from  $10^\circ$  to a maximum of  $47^\circ$  and then decreases to about  $10^\circ$ . On the outward path, the distance from the Sun will reach  $10^\circ$  on January 2, 1974, and thereafter will increase to a maximum of about  $68^\circ$  by February 17, decrease till about mid-June and then increase again. So there will be further opportunity for studying changes in the dimensions of the comet on the outward path.

The dates and positions in the orbit given here are based on the ephemeris by D. K. Yeomans.

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## Y-ray Bursts and the Diffuse Flux of Cosmic X-rays

MANY attempts have been made to explain the diffuse cosmic X-ray flux in terms of contributions from a large number of discrete extragalactic sources. The nature of these sources has remained obscure; the steady emission from normal galaxies, such as our own, fails by more than an order of magnitude to explain the intensity of the diffuse flux<sup>1</sup>. The spectrum of the diffuse flux can be approximated to a power law

$$n(\epsilon) \sim 30 \epsilon^{-2} \text{ photons cm}^{-2} \text{ s}^{-1} \text{ sr}^{-1} \text{ keV}^{-1} \quad (1)$$

( $\epsilon$  in keV) for values of  $\epsilon$  from 1 keV to 1 MeV. This poses a further problem because the spectrum is much harder than that of the discrete extragalactic sources which have been detected. Klebesadel *et al.*<sup>2</sup> have recently reported observations of bursts of radiation with hard spectra which extend to photon energies greater than 1 MeV. Here I shall show that, whatever is the nature of the sources of these bursts, they are unlikely to make a significant contribution to the diffuse flux.

Between July 1969 and July 1972 Klebesadel *et al.* observed sixteen bursts each with an integrated intensity at the Earth greater than  $10^{-5} \text{ erg cm}^{-2}$  in the spectral region between

0.3 MeV and 1.5 MeV. In the same energy interval the diffuse flux has an intensity of  $8 \times 10^{-8} \text{ erg cm}^{-2} \text{ s}^{-1} \text{ sr}^{-1}$ . In the absence of any information as to the distances to the sources of the bursts a relationship may be derived between their number density,  $n$ , and their strength  $S$ . I shall assume that space is Euclidean and that all the sources have the same intrinsic strength.

I shall take the mean interval between bursts from an individual source to be  $\tau$ . (No source was observed to give more than one burst during the 3 yr of observation.) Then the observed bursts, with a rate of  $1.6 \times 10^{-7} \text{ s}^{-1}$ , must have been produced by sources within a distance  $r_1$ , where  $r_1$  is given by

$$(n/\tau)4/3(\pi r_1^3) \sim 1.6 \times 10^{-7} \text{ s}^{-1} \quad (2)$$

In each case the energy received at the Earth was greater than  $10^{-5} \text{ erg cm}^{-2} \text{ s}^{-1}$  so

$$S/4\pi r_1^2 \sim 10^{-5} \text{ erg cm}^{-2} \quad (3)$$

Eliminating  $r_1$  from equation (2) and equation (3) it turns out that

$$(n/\tau) S^{3/2} \sim 6 \times 10^{-14} \text{ erg}^{3/2} \text{ s}^{-1} \text{ cm}^{-3} \quad (4)$$

I shall now calculate the contribution which these sources could make to the diffuse flux. It will be necessary to consider two cases.

Case 1. The sources lie outside our Galaxy, and presumably in external galaxies. To a first approximation the total contribution from these sources may be calculated by considering space to be Euclidean and integrating out to the Hubble radius,  $r_H$ . The contribution,  $dI$ , to the diffuse flux from sources at a distance,  $r$ , from the Earth is given by

$$dI \sim (n/\tau)(S/4\pi r^2) r^2 dr \text{ erg cm}^{-2} \text{ s}^{-1} \text{ sr}^{-1} \quad (5)$$

Integrating out to a distance,  $r_H$ , yields

$$I \sim (n/\tau)(S/4\pi) r_H \text{ erg cm}^{-2} \text{ s}^{-1} \text{ sr}^{-1} \quad (6)$$

Putting  $I = 8 \times 10^{-8} \text{ erg cm}^{-2} \text{ s}^{-1} \text{ sr}^{-1}$  and  $r_H = 10^{28} \text{ cm}$  gives

$$(n/\tau)S \sim 10^{-34} \text{ erg s}^{-1} \text{ cm}^{-3} \quad (7)$$

From equation (4) and equation (7)

$$S \sim 4 \times 10^{41} \text{ erg}$$

This energy release is acceptable since it represents less than  $10^{-8}$  of that expected in a supernova explosion. But I shall show that the number density of the sources is unreasonably large.

From equation (7) we find

$$n/\tau \sim 3 \times 10^{-76} \text{ cm}^{-3} \text{ s}^{-1}$$

If  $n_g$  is the number density of galaxies in the Universe then the rate of bursts per galaxy,  $v_g$ , is given by

$$v_g \sim n/n_g \tau \quad (8)$$

Putting  $n_g \sim 5 \times 10^{-75} \text{ cm}^{-3}$  gives

$$v_g \sim 6 \times 10^{-2} \text{ s}^{-1}$$

This is clearly inconsistent with the observations.

Case 2. The sources are distributed throughout our own Galaxy and in external galaxies. In this case, if the number density of sources inside our own Galaxy is  $n \text{ cm}^{-3}$ , then the mean density outside will be  $\beta n \text{ cm}^{-3}$  where  $\beta$  is the fraction by volume of the Universe occupied by galaxies. If the mean volume of a galaxy is taken as  $3 \times 10^{67} \text{ cm}^3$ ,  $\beta \sim 10^{-8}$ . The equation corresponding to equation (6) in the previous case is

$$I \sim \int_0^{r_g} (n/\tau)(S/4\pi)dr + \int_{r_g}^{r_H} (\beta n/\tau)(S/4\pi)dr \quad (9)$$

where  $r_g$  is a typical dimension of our Galaxy.

Equation (9) can be written as

$$I \sim (n/\tau)(S/4\pi)r_g [1 + (\beta r_H/r_g)]$$

Putting  $r_g \sim 10^{23} \text{ cm}$  yields, corresponding to equation (7) in the previous case,

$$(n/\tau)S \sim 10^{-29} \text{ erg s}^{-1} \quad (10)$$

From equation (4) and equation (10)

$$S \sim 4 \times 10^{31} \text{ erg} \\ \text{and } (n/\tau) \sim 3 \times 10^{-61} \text{ cm}^{-3} \text{ s}^{-1}$$

Now, since only one burst was observed from each direction,

$$\tau > 10^8 \text{ s}$$

Thus,

$$n > 3 \times 10^{-53} \text{ cm}^{-3}$$

This density far exceeds the mean density of stars in the Galaxy which is  $\sim 10^{-56} \text{ cm}^{-3}$ .

These arguments do not exclude the suggestion made by Apparao<sup>3</sup> and Tucker<sup>4</sup> that the diffuse flux may be due to the superposition of radiation bursts occurring in the early stages of supernova outbursts. By analogy with equation (6) the total energy release in each burst,  $S$ , which is required to explain the diffuse flux is given by the equation

$$Sn_g v_g r_H \sim 4\pi I \quad (11)$$

where  $v_g$  is the frequency of supernova outbursts per galaxy.

The detectability of these bursts decreases as their duration,  $t_s$ , increases. In the spectral region from 1 keV to 10 keV there have been many sky surveys, using detectors on rockets, which would detect fluxes down to  $\sim 3 \times 10^{-10} \text{ erg cm}^{-2} \text{ s}^{-1}$ . In such a survey, a supernova outburst would appear as a normal source provided  $t_s > 10 \text{ s}$ , and would be detectable out to a distance  $r_1$  where

$$(S/4\pi r_1^2 t_s) \sim 3 \times 10^{-10} \text{ erg cm}^{-2} \text{ s}^{-1} \quad (12)$$

The frequency,  $v$ , of supernova outbursts occurring in this volume is given by

$$v \sim (4\pi r_1^3/3)n_g v_g \quad (13)$$

The number of supernovae which should be detectable in any sky survey is given by the product ( $vt_s$ ). On a subsequent survey these sources would have disappeared but would have been replaced by a similar number of different sources. No variable extragalactic sources of this type have been reported so

$$vt_s < 1$$

Equations (1), (12) and (13) yield

$$t_s > 5 \times 10^5 \text{ s} \sim 5 \text{ d}$$

With a detector on a satellite it should be possible to make X-ray observations within a day of a supernova outburst; for a supernova in a nearby galaxy a detector with only modest sensitivity can provide a crucial test of this theory of the origin of the diffuse X-ray flux.

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## Chemical Evidence for Lunar Melting and Differentiation

AN important chemical characteristic of lunar samples is the high concentration of refractory elements, coupled with depletion of volatile elements in comparison with primitive solar nebula element abundances<sup>1,2</sup>. The concentration of many refractory elements (such as rare earths) in lunar samples frequently equals or exceeds those in terrestrial crustal samples. It is clear that such high abundances are not typical of the whole Moon. For example, the contents of the heat-producing elements, K, U and Th in the surficial rocks are sufficient to generate the observed heat flow from a thin outer zone. If such concentrations persisted at depth, the Moon would be molten. Thus the concept of an outer zone of the Moon enriched in refractory elements has gained general acceptance. Two basic models have been proposed to account for these observations: (a) melting and differentiation of the outer layers, or of the whole Moon, to provide both a lunar crust enriched in "incompatible" elements, and an alumina rich highland crust<sup>3</sup>; (b) primary accretion of an outer layer enriched in refractory elements (heterogeneous accretion model)<sup>4,5</sup>.

Ratios of volatile to involatile elements (such as K/Zr, K/La) are constant for many elements both in mare and highland samples<sup>6,7</sup>. It is particularly significant that many elements of varying volatility show good correlations. Such observations place constraints on heterogeneous accretion models, which accrete a highland crust more depleted in volatile elements than the deeper source regions of the mare-basalts. In this communication I examine further the relationship between the highland crustal composition and that of the whole Moon, to see if some choice can be made between the two models.

Tables of elemental abundances both for the highland crust and the whole Moon have recently been proposed. A two component model for the chemical composition of the whole Moon has been suggested by Wänke *et al.*<sup>7</sup>. One component comprises 69% of a refractory or high temperature condensate, similar in composition to the white Ca-Al rich inclusions in the Pueblito de Allende type 2 carbonaceous chondrite. The other component (31%) is of unfractionated type 1 carbonaceous chondrite composition, contributing all of the volatile elements. The resulting composition is given in the first column of Table 1. This is a high Al composition. The question of the abundance of Al in the lunar interior is still unresolved<sup>8</sup>. But the conclusions I draw here are not sensitive to the concentration of aluminium, because none of the trace elements considered occupy Al lattice sites. The overall average composition of the lunar highlands has been proposed by Taylor *et al.*<sup>9</sup>, using established interelement correlations, coupled with data from the lunar orbiter gamma ray Th values and the XRF Al/Si and Mg/Si ratios. This composition (Table 1) can be represented by a mixture of 69% highland basalt (anorthositic gabbro) and 31% low K Fra Mauro basalt.

From seismic evidence<sup>10</sup> a lunar crust about 65 km thick has been postulated in the Oceanus Procellarum region. The seismic velocity data for the lower 45 km are consistent with anorthositic gabbro. The thickness in other areas is not yet established but is assumed to be the same or greater in non-mare regions. A crust of anorthositic gabbro occupying about 10% of the volume of the Moon is assumed. The conclusions which follow are not sensitively dependent on this assumption.

The compositional data for the highland crust are based on surface sampling and orbital data. Is this representative of a thick crust, or merely of a surficial layer? The intensive cratering history of the highland crust suggests that it is well mixed at depth. In Fig. 1 I show the eleven

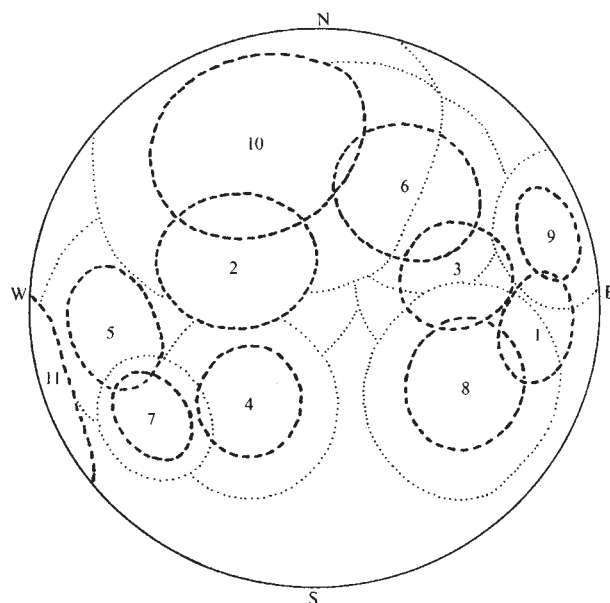


Fig. 1 Lunar map showing main outer rims (---) of mare basins and extent of ejecta blankets (....). The relative ages are indicated by the numbers. 1, Fecunditatis; 2, South Imbrium; 3, Tranquillitatis; 4, Nubium; 5, Procellarum; 6, Serenitatis; 7, Humorum; 8, Nectaris; 9, Crisium; 10, Imbrium; 11, Orientale. (Adapted from Wilshire and Jackson<sup>13</sup>.)

observed mare basins on the visible face of the Moon. These formed by impact in the highland crust before the mare basalt floods and are typically several hundred kilometres in diameter. The depth to which mixing has occurred is unknown, but extensive overturning of the crust has happened during these collisions. The impacts occurred late in the development of the present highlands terrain and were preceded by an intense bombardment, producing impact craters up to 180 km in diameter (such as Clavius). Any pre-existing layering or vertical heterogeneity in the crust would be destroyed by this cratering, and the highland crust is probably uniform in composition with depth. Lateral variations in composition on a scale of hundreds of kilometres might persist, although the mare basin formation would have destroyed any finer scale heterogeneity.

Table 1 Composition of the Whole Moon and Highland Crust, and the Percentage of Elements in the Crust Relative to the Whole Moon

|           | Whole Moon <sup>7</sup> | Highland crust <sup>9</sup> | Percentage in crust |
|-----------|-------------------------|-----------------------------|---------------------|
| Si %      | 15.0                    | 19.7                        | 13                  |
| Ti %      | 0.70                    | 0.41                        | 5.9                 |
| Al %      | 12.0                    | 13.2                        | 11                  |
| Fe %      | 8.6                     | 4.9                         | 5.7                 |
| Mg %      | 8.9                     | 5.12                        | 5.8                 |
| Ca %      | 12.8                    | 9.9                         | 7.7                 |
| Na p.p.m. | 1,770                   | 2,500                       | 14.1                |
| K p.p.m.  | 250                     | 800                         | 32                  |
| Cs p.p.m. | 0.04                    | 0.1                         | 26                  |
| Rb p.p.m. | 0.87                    | 2.3                         | 26                  |
| Ba p.p.m. | 33                      | 144                         | 44                  |
| Sr p.p.m. | 93                      | 190                         | 20                  |
| Eu p.p.m. | 0.81                    | 1.7                         | 21                  |
| La p.p.m. | 3.5                     | 12                          | 34                  |
| Y p.p.m.  | 24                      | 32                          | 13                  |
| Th p.p.m. | 0.32                    | 1.8                         | 56                  |
| U p.p.m.  | 0.086                   | 0.46                        | 54                  |
| Zr p.p.m. | 67                      | 156                         | 23                  |
| Nb p.p.m. | 4.8                     | 10.8                        | 23                  |
| Sc p.p.m. | 81                      | 7.7                         | 0.95                |
| Mn p.p.m. | 700                     | 600                         | 8.6                 |
| V p.p.m.  | 440                     | 25                          | 0.57                |
| Cr p.p.m. | 1,100                   | 750                         | 8.8                 |

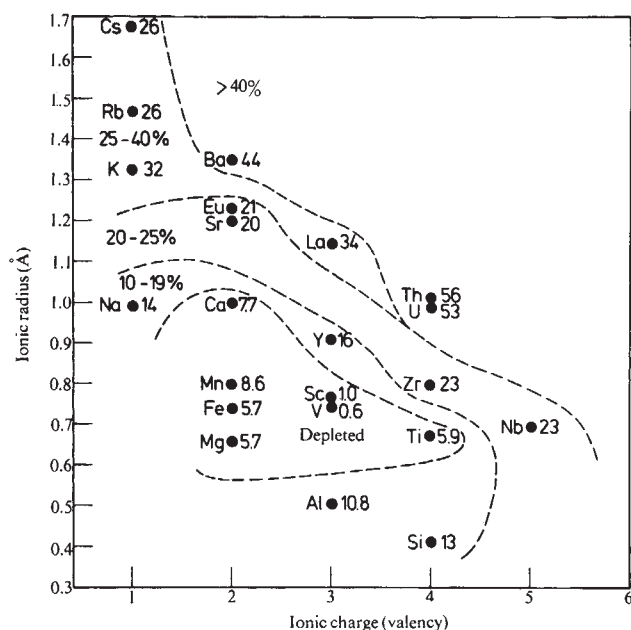


Fig. 2 Relative enrichment or depletion of chemical elements in the lunar highlands<sup>9</sup>, relative to whole Moon abundances<sup>7</sup>, shown as a function of ionic radius<sup>14</sup> and valency. Note the progressive enrichment of elements as size and valency differ from those of Mg and Fe, major constituents in the lunar interior. The boundary between depletion and enrichment is taken as 10%, the assumed volume of the lunar crust.

On the basis that the composition given in the second column of Table 1 is typical of the lunar crust, I have calculated the percentage of each element in the crust relative to the whole Moon (third column of Table 1). These values are shown on a diagram (Fig. 2) relating ionic radius and valency. Elements which are similar in size and valency to  $\text{Fe}^{2+}$  and  $\text{Mg}^{2+}$  are depleted in the crust. These cations are assumed to be major constituents on the lunar interior, and elements of similar chemical character are retained in crystal lattice sites occupied by them. As the size or valency of the elements differs from those of  $\text{Fe}^{2+}$  and  $\text{Mg}^{2+}$ , there is a progressive enrichment in the lunar crust, illustrated by the contour lines of equal enrichment in Fig. 2. Such a pattern is consistent with crystal-liquid fractionation since the entry into or rejection of lithophile ions from silicate lattices is dependent on size and valency *inter alia*. For the elements discussed here, the other factors, such as bond type (ionic-covalent) or crystal field effects, are less important. I note the extreme depletion of the small trivalent cations  $\text{Sc}^{3+}$  and  $\text{V}^{3+}$  in the highland crust (Table 1, Fig. 2). These are much more depleted than  $\text{Cr}^{3+}$  (8.8%, Table 1), indicating that they may be preferentially retained in separate mineral phases in the lunar interior.

There is no simple correlation with volatility. Anderson<sup>8</sup> gives the following order of increasing volatility: Sc, Zr, Nb, Y, Hf, Ti, Ca, Th. The data in Table 1 and Fig. 2 show, for example, that Th is much more enriched than Zr or Sc, the reverse of that predicted on the basis of a condensation sequence. Thus it is clear that the overriding influence in the partitioning of elements into the highlands has been that of solid-liquid, not solid-gaseous equilibria. The whole Moon, or at least the portion affected by melting, is depleted in volatile elements, relative to primordial solar nebula abundances (as represented by the composition of type 1 carbonaceous chondrites). The effect under discussion here occurs during post-accretion melting and differentiation operating on a bulk composition already enriched in refractory and depleted in volatile elements.

How much of the Moon is involved in this process?

The values calculated in Table 1 assume that the whole Moon was involved but the conclusions would not differ if a smaller volume was melted. Geochemical balance calculations place some limits. Thus more than 50% of the U and Th and 44% of the Ba in the whole Moon is concentrated in the highland crust, implying extremely efficient upward concentration of the elements. Some of the U and Th must have been retained at depth to provide both the observed abundances (typically 0.25 p.p.m. U and 1.0 p.p.m. Th) in the mare basalts (produced by partial melting at depths in the range 200 to 500 km)<sup>11</sup> and to provide a source of heat for the melting itself, spread over the time span of 3.8 to  $3.2 \times 10^9$  yr. The remaining U and Th in the Moon (using the Taylor-Wänke model compositions) would be accounted for in the zone from 200 to 500 km depth if this region contains abundances of 0.1 p.p.m. U and 0.4 p.p.m. Th. Such abundances are close to those in eucritic meteorites, and would produce temperatures, from the calculations of Wänke *et al.*<sup>15</sup>, appropriate for the production of lunar basalts about  $3.8 \times 10^9$  yr ago. Thus it is probable that most of the U and Th in the Moon was either in the highland crust, or in the source regions of the mare basalts at  $3.8 \times 10^9$  yr ago, and that most of the Moon was involved in the melting process.

In summary, it seems that the general features of lunar highland chemistry can be explained by melting and differentiation of a Moon (already depleted in volatiles). The lack of correlation with element volatility constitutes further evidence against the accretion of the outer layer of the Moon from more refractory material than that comprising the interior. This reinforces the evidence of uniformity of volatile/involatile element ratios<sup>6,7</sup> between highland and mare samples.

A further consequence of the uniformity of many element ratios (such as Zr/Nb, Ba/Rb)<sup>16</sup> may be noted. Small scale melting episodes would be expected to produce variations in these ratios in different regions. The uniformity observed indicates widespread homogeneity. This is a surprising result, because the absence of water and volatiles would tend to preserve any original heterogeneities. One fact which might be cited as evidence for a relative enrichment of refractory elements in the highlands is the K/U ratio. This is lower ( $\sim 1,500$ ) in highland samples than in mare basalts ( $\sim 2,500$ )<sup>12</sup>. This enrichment of U relative to K could be due to accretion of more refractory material. But it may also be a consequence of crystal-liquid fractionation. It can be seen from Table 1 and Fig. 2 that U (and Th) are the most highly enriched elements in the highlands, and are thus concentrated upwards to a greater extent than potassium. Additional evidence of crystal-chemical rather than volatility dependence comes from the much more uniform values observed for Rb/U ratios, although Rb is more volatile than K. Furthermore, elements which are more refractory than U (such as Zr) do not show such variations with K between mare and highlands samples.

I conclude that the relative enrichment of the elements in the lunar highlands shown in Fig. 2 is not related to differences in volatility, but is consistent with widespread melting and crystal fractionation in the Moon, acting on material already depleted in the volatile elements.

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## Stratospheric Nitric Oxide from Infrared Spectra

AERONOMERS have generally admitted that the abundance of nitric oxide is small in the chemosphere. Its detection and even more its measurement have been regarded as a difficult task. The importance of this species in atmospheric chemistry, however, makes the effort to measure it worthwhile. Data have been obtained up to now by using the resonant scattering of solar ultraviolet radiation<sup>1</sup> and the ionizing blunt probe technique<sup>2</sup> in the mesosphere and lower thermosphere. Here we report the first determination of the vertical distribution of the abundance of nitric oxide in the stratosphere. The method involves absorption spectrometry in the 5.2  $\mu\text{m}$  band of NO using the setting Sun as a light source.

Because the postulated amount of absorber in the optical path is small and relatively fast wavelength scanning is necessary, the spectrometer used had to have the double advantage of large light input and high resolving power. The instrument, one of the grid spectrometers developed by Girard<sup>3</sup>, had a focal length of 60 cm and was equipped with a 6 cm  $\times$  6 cm Yvon et Jobin grating ruled at 60 grooves  $\text{mm}^{-1}$  and used in the sixth order. The foreoptics was a 4.2 m focal length modified Cassegrain telescope of 32 cm diameter. The detector was a liquid nitrogen-cooled 'SAT' InSb photo element. An instrumental profile of 0.1  $\text{cm}^{-1}$  halfwidth was achieved with a square grid of 18  $\times$  18 mm having a step of 0.2 mm. The instrument was mounted on a Sun seeker equipped with the electronics used for the synchronous detection of the detector output with the voltage controlled oscillators and with the transmitter used in the telemetry radio link. The 320 kg gondola was launched from Aire sur l'Adour in the afternoon of May 14, 1973, with an 11.6  $\times$  10<sup>6</sup> foot<sup>3</sup> 'Winzen' balloon.

Recording of the solar spectrum from an altitude of 40 km started when the solar zenith angle was 86°. The spectra then showed little structure partly on account of solar CO absorption lines<sup>4</sup>. As shown in Fig. 1, telluric absorptions become more

**Table 1** Data on NO

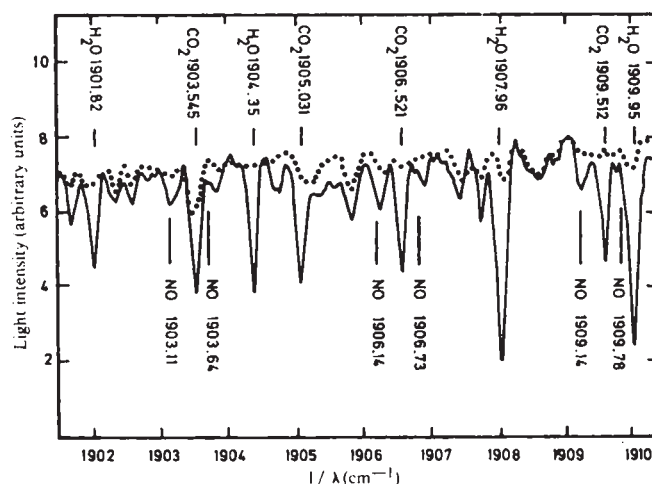
| Altitude (km) | NO number density ( $\text{cm}^{-3}$ ) | NO mixing ratio by volume       |
|---------------|----------------------------------------|---------------------------------|
| 37.5          | $(5 \pm 2) \times 10^8$                | $(4.4 \pm 1.8) \times 10^{-9}$  |
| 34.5          | $(6 \pm 2) \times 10^8$                | $(3.4 \pm 1.2) \times 10^{-9}$  |
| 31.5          | $(8 \pm 2) \times 10^8$                | $(2.8 \pm 0.7) \times 10^{-9}$  |
| 28.5          | $(8 \pm 2) \times 10^8$                | $(1.8 \pm 0.5) \times 10^{-9}$  |
| 25.5          | $(7.4 \pm 2) \times 10^8$              | $(9.7 \pm 2.2) \times 10^{-10}$ |
| 22.5          | $(6.5 \pm 2) \times 10^8$              | $(5.7 \pm 1.8) \times 10^{-10}$ |
| 19.5          | $(4.6 \pm 2) \times 10^8$              | $(2.5 \pm 1.1) \times 10^{-10}$ |
| 16.5          | $(2 \pm 1) \times 10^8$                | $(6.7 \pm 3.5) \times 10^{-11}$ |

and more pronounced for solar zenith angles larger than 90° because of the increase in the optical path, reaching lower and lower altitudes in the stratosphere. Absorption lines of NO, CO<sub>2</sub> and H<sub>2</sub>O were predominant. CO<sub>2</sub> and H<sub>2</sub>O lines were identified on the basis of the AFCRL infrared compilation<sup>5</sup> and for NO other laboratory data were used<sup>6</sup>. The amount of NO in the optical path was deduced from the integrated line absorption cross sections computed<sup>7</sup> from the band intensity<sup>8</sup>. The number density of NO against altitude was inferred from the total amounts measured by division of the atmosphere into successive layers, each 3 km thick.

The values are given in Table 1 with the corresponding mixing ratios. The limits of error indicated correspond to the maximum scatter of the original thirty data points taken for grazing ray altitudes from 38 to 15 km. The results are represented in Fig. 1 with other experimental data. On June 6, 1973, a similar spectrometer, previously flown on a Caravelle aircraft<sup>9</sup>, was used on Concorde 001 in the same spectral region and with the same resolving power. The integrated amounts of NO measured in this latter case along the optical path from 15.2 km are  $(3 \pm 1) \times 10^{16}$  and  $(5.5 \pm 1) \times 10^{16}$  moles  $\text{cm}^{-2}$  at solar zenith angles of 88° and 90°, respectively. The results are, within experimental error, in agreement with the data presented in Fig. 2.

Several conclusions can be drawn from the new appearance of the chemospheric NO distribution resulting from the addition of the new data reported here. As suggested by Nicolet<sup>10</sup>, a stratospheric source of NO must exist. The photodissociation of NO is a sufficiently large sink in the low mesosphere to compensate for the downward mesospheric and upward stratospheric NO fluxes<sup>11</sup>. The sum of the measured amounts of NO and NO<sub>2</sub> (ref. 12) indicates a large sink of NO<sub>x</sub> below 25 km at mid-latitudes.

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**Fig. 1** Intensity of solar radiation recorded at an altitude of 40 km against wavenumber in  $\text{cm}^{-1}$ . . . ., Spectrum at a solar zenith angle of 86°; —, spectrum at a zenith angle of 94°. The absorption lines due to stratospheric NO, CO<sub>2</sub> and H<sub>2</sub>O are indicated.

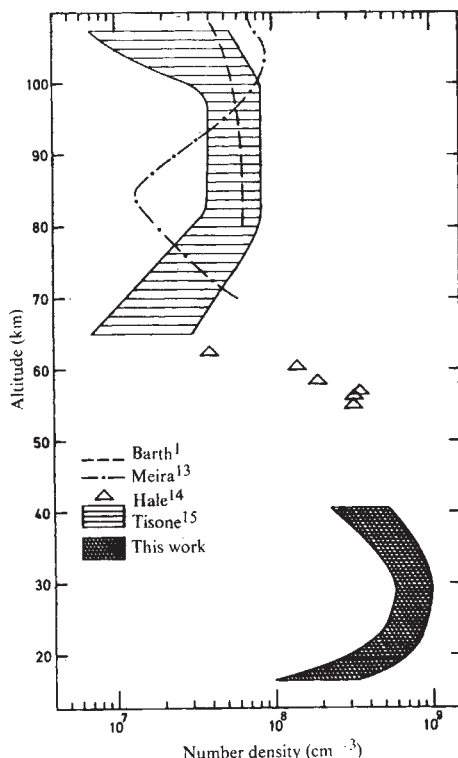


Fig. 2 Experimental values of the number density of nitric oxide against altitude in the chemosphere.

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## Radio Detection of Low Energy Electrostatic Sparks

WE have carried out laboratory and outdoor studies to see whether radio observations with a tuned loop aerial can be used to monitor low energy electrostatic sparks and to separate spark discharges from corona-type discharges. Since corona is in general non-incendive in inflammable hydrocarbon air mixtures—but sparks may be incendive if sufficiently energetic—the observation of electromagnetic radiation from a discharge could form a simple technique for monitoring potential ignition hazards.

The types of discharge in which we have been interested are those which could provide a mechanism for the ignition of explosions in large oil tankers. Electrostatically charged mists can be generated in cargo holds by tank cleaning operations with high pressure water jets and by sloshing of water in part ballasted tanks due to ship motion. It is thought that electrostatic sparks may occur when conducting bodies such as slugs of water approach projections into the tank space where there is a concentration of electric field, or when such bodies leave these projections and move to the tank wall<sup>1</sup>. We have simulated this type of localized electrostatic discharge by discharges between a variety of isolated smooth metal electrodes and a plane metal electrode (see Fig. 1).

These bodies were connected to a high voltage supply by a very high impedance lead—around  $10^{11} \Omega$ . The large RC time constant of this lead, compared with radio frequencies, ensured that only the electrostatic charge on

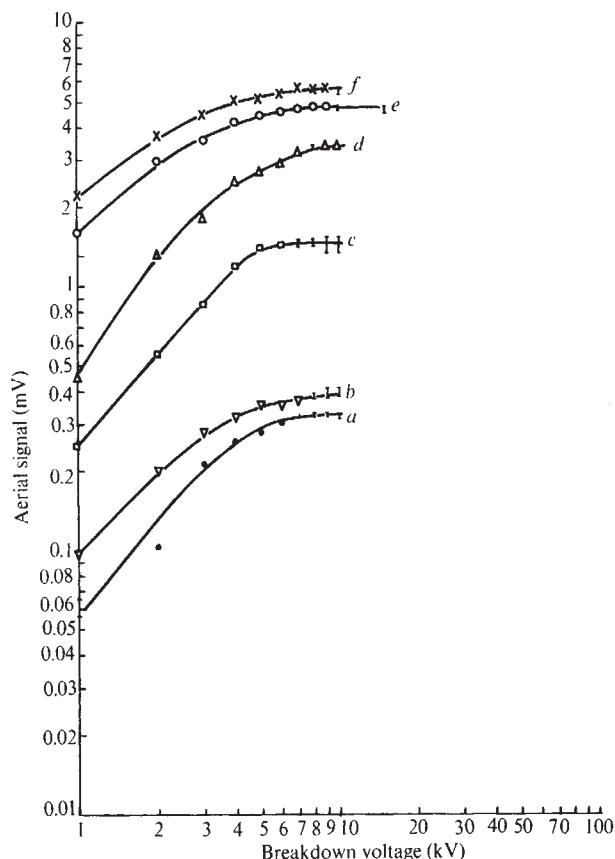


Fig. 1 Variation of 38 MHz aerial signal with breakdown voltage for various isolated bodies. Discharges were between a plane metal electrode and a, sphere 19 mm in diameter; b, sphere 25 mm in diameter; c, sphere 50 mm in diameter; d, spherically ended cylinder, 25 mm in diameter, 125 mm long; e, sphere 125 mm in diameter; f, spherically ended cylinder, 25 mm in diameter, 250 mm long. Spark gap at ground level. Aerial at 10 m range and 2.4 m elevation. Aerial bandwidth about 2 MHz.



the conducting body could contribute to a discharge from the body, and that only the conducting body and the discharge channel could act as radiating aerial elements. Sparking rates were kept low, around  $1 \text{ s}^{-1}$ , to minimize interaction of residual ionization on successive sparks.

Observations of electromagnetic radiation were made at 38 MHz using a tuned loop aerial with a bandwidth of about 2 MHz. This frequency was chosen mainly for convenience—some observations have also been made at 10, 150 and 3,000 MHz. The pulse of r.f. oscillation induced in the tuned loop aerial was monitored by a simple wide band integrated circuit amplifier (using Plessey SL 611 units) followed by a transistor detector. The transfer characteristics of these circuits followed a roughly square law variation of gain with signal amplitude, from about 52 dB at 0.1 mV input to 72 dB at 1 mV input. Experimental observations were made in a fairly flat outdoor area well away from large buildings and structures which might produce reflexion and interference effects (see Fig. 1).

The results of our experiments are shown in Fig. 1. Our studies show the following.

(1) The aerial signal at 38 MHz rises with breakdown voltage at low voltages, and with all the bodies studied reaches a plateau value between 6 and 10 kV.

(2) For breakdown voltages up to about 6 kV there is little variation in signal amplitude between successive individual sparks. For breakdown voltages above about 6 kV the amplitudes of signals may vary appreciably from spark to spark—although the mean value seems to remain steady at around the plateau value.

(3) Sparks from larger bodies give rise to larger aerial signals at any particular voltage—but there is no simple correlation between signal amplitude and the energy or coulombs discharged (even after taking account of the increase in capacitance of bodies near a conducting surface).

(4) For sphere to plane sparks, the aerial signals vary roughly linearly with distance of the aerial from the spark source—confirming that the observations relate to electromagnetic radiation.

(5) Several observations confirm that signals generated by metal to metal spark discharges are comparable to those for metal to water discharges—so long as the speed of approach of the discharge surfaces is fast enough (above about  $0.5 \text{ m s}^{-1}$ ) to avoid breakdown by mechanical instability<sup>2</sup> of the water surface.

(6) Even weak sparks can be observed quite easily. For example, breakdown of the 19 mm sphere to plane gap at 1 kV involved the discharge of only 3 nC of charge and about 2  $\mu\text{J}$  of energy.

Observations on the radio emission characteristics of positive and negative point corona discharges from isolated bodies were also made in outdoor studies. The impedance of the high voltage supply lead was reduced to around  $10^7 \Omega$  and a sharp 30 mm high projection was placed on the plane electrode beneath the bodies. At the voltages used in the above studies no radio signals were observed with either negative or positive point corona. Only for much higher voltages, large bodies and high discharge currents of positive point corona were any signals observed—these were still fairly small compared to those observed with low energy sparks. For example, with a sphere 125 mm in diameter and a 30 mm gap to the 30 mm projection, the amplitude of the radio signals varied from about 0.1 mV at 32 kV and 0.1 mA discharge current, to 0.25 mV at 47 kV and 0.3 mA discharge current.

Among the features of the radio emission characteristics of electrostatic discharges from isolated bodies worthy of further investigations are the reason for the appearance of a plateau in the variation of signal amplitudes with breakdown voltage and the source of the spark to spark signal variations at high breakdown voltages. These features and the optimum frequencies to use in practical studies might

be revealed by examination of the radio emission spectrum. Measurements of signal amplitude may give some information on the smallest size body which could be involved in individual discharges.

Our observational technique has been used in shipboard studies for the International Chamber of Shipping to find the conditions in which electrostatic sparks occur during tank washing operations and during sloshing in part-ballasted cargo tanks in oil tankers and combination carriers.

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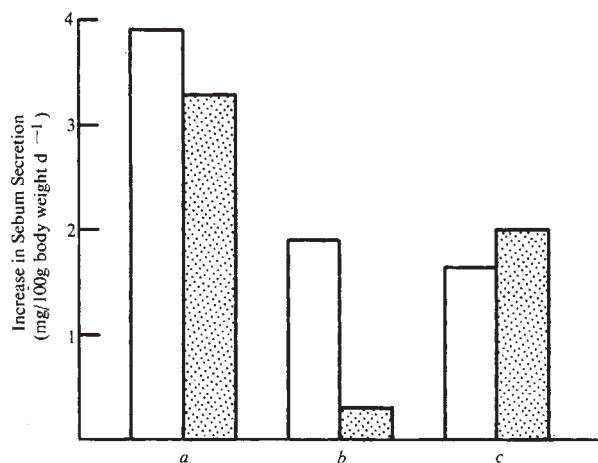
## BIOLOGICAL SCIENCES

### Possible Role of MSH in the Mammal

PITUITARY control of sebaceous gland activity has generally been assumed to be a function of the anterior lobe. The possibility that the neurointermediate (NI) lobe is involved was first suggested when we found that its removal led to a decrease in sebum secretion to a level almost as low as that after total hypophysectomy<sup>1</sup>. We concluded that the NI lobe is responsible for a sebotrophic hormone. Several hormones are produced by the NI lobe and we now present evidence that this sebotrophic hormone is related to the melanocyte-stimulating hormones (MSH) from the pars intermedia.

In the first series of experiments we examined the effects of neurointermediate lobe hormones on sebum secretion. Adult female Wistar rats received subcutaneous injections of the various hormones twice daily in 0.9% NaCl and after 2 weeks the rate of sebum secretion was measured<sup>2</sup>. Daily treatment with 50  $\mu\text{g}$  synthetic  $\alpha$ -MSH produced highly significant increases in sebum secretion in both intact ( $P < 0.001$ ) and hypophysectomized ( $P < 0.001$ ) rats (Table 1). On the other hand, daily treatment with either 1.5 or 4.5 U of oxytocin ('Syntocinon', Sandoz) produced no change in the sebum secretion rate of hypophysectomized rats (Table 1). Similarly, treatment with 1.5 U vasopressin ('Pitressin', Parke-Davis) had no effect, but there was a small increase ( $P < 0.02$ ) in sebum secretion after treatment with 4.5 U (Table 1). This small sebotrophic effect may have been due to contamination with MSH: 'Pitressin', unlike 'Syntocinon' which is synthetic, is prepared from the pig posterior pituitary and we have found that one unit of vasopressin contains 100–120 units of MSH activity (equivalent to 0.5–0.6  $\mu\text{g}$  of  $\alpha$ -MSH). These results suggest that the sebotrophic hormone is related to an MSH from the pars intermedia.

We tested this further by comparing the sebotrophic and MSH activity of an NI lobe extract. The NI lobe was separated from acetone-dried pig pituitaries and an aqueous extract prepared (a). A second extract (b) was prepared from the anterior lobe powder, after removal of known anterior pituitary hormones, by the method of Lorincz<sup>3</sup>. The original acetone washings were dried (c) and assayed since any sebotrophic material could have been removed by the acetone. The sebotrophic activity of these extracts was then measured in testosterone treated castrated-hypophysectomized rats.



**Fig. 1** Effect of different pituitary extracts on sebum secretion. These extracts were prepared from waste fractions discarded during the commercial preparation (Organon Laboratories Ltd) of pituitary hormones. Fresh pig pituitaries were dried with acetone and separated into anterior and neurointermediate lobes. The neurointermediate lobe powder was extracted with distilled water (100 ml g<sup>-1</sup> powder) for 60 min at room temperature and then centrifuged at 12,000 r.p.m. The supernatant was freeze-dried (a). The anterior lobe powder was used for the commercial preparation of known anterior pituitary hormones (Organon Laboratories Ltd). The residue that remained after extraction with acetic acid was suspended in water and the pH adjusted to 8.6 and filtered. The pH of the filtrate was adjusted to 3.0 with dilute HCl and cooled for 2 d at 8° C. The precipitate that formed was collected and freeze-dried (b). This procedure is similar to that originally described by Lorincz<sup>3</sup>. The acetone washings were also freeze-dried (c). Fresh pig pituitary tissue (200 g) yielded 5.5 g a, 0.6 g b, and 4.0 g c. The sebotropic activity of these extracts was measured in castrated-hypophysectomized rats treated with 0.2 mg testosterone per 100 g body weight. Subcutaneous injections of the extracts (8 mg d<sup>-1</sup>) in saline were given for 2 weeks and the rate of sebum secretion then measured<sup>2</sup>. The sebotropic activity (white areas) is shown as the increase in sebum secretion (mg per 100 g body weight d<sup>-1</sup>) over that of the control rats. The MSH content of the extracts a, b and c was assayed<sup>4</sup>, and the amount of sebotropic activity of each extract that could be accounted for by its MSH content is shown (hatched areas).

The rats were divided into four groups and received daily subcutaneous injections of 8 mg of the extract in 0.9% saline. Treatment was continued for 2 weeks and the rate of sebum secretion then measured. Sebotropic activity, defined as the increase in sebum secretion in mg per 100 g body weight d<sup>-1</sup> greater than that of the control rats, was found in all three extracts (Fig. 1). But when this was expressed in relation to the starting material (Fig. 2) it can be seen that most of the activity occurred in extract a from the NI lobe. There was a smaller amount in the acetone washings (c) and negligible amounts in the anterior lobe extract (b). The high MSH contents of extracts a and c (78,000 and 19,000 units mg<sup>-1</sup> respectively) could account for their sebotropic activity (Fig. 1). Extract b contained very little MSH activity (2,400 units mg<sup>-1</sup>) and sebotropic activity of this extract is probably due to residual anterior pituitary trophic hormones (A. J. T., W. M. Goodale, and S. S., unpublished).

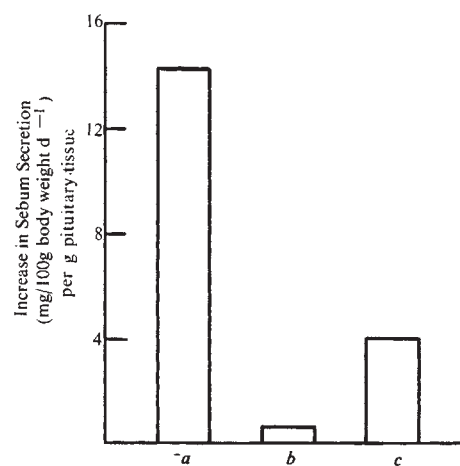
These observations further support the idea that the sebotropic hormone from the NI lobe is an MSH. If this is true then administration of MSH should correct the effects of the removal of the NI lobe on sebaceous gland function. After removal of the NI lobe in male rats the rate of sebum secretion was significantly lower than that found in sham-operated and intact rats (Fig. 3). After daily treatment with 30 µg α-MSH per 100 g body weight for 2 weeks the rate of sebum secretion in the posterior hypophysectomized rats increased and was restored to the normal levels found in the sham-operated and intact rats.

The physiological significance of MSH in the mammal has not been established. In amphibia MSH is concerned with

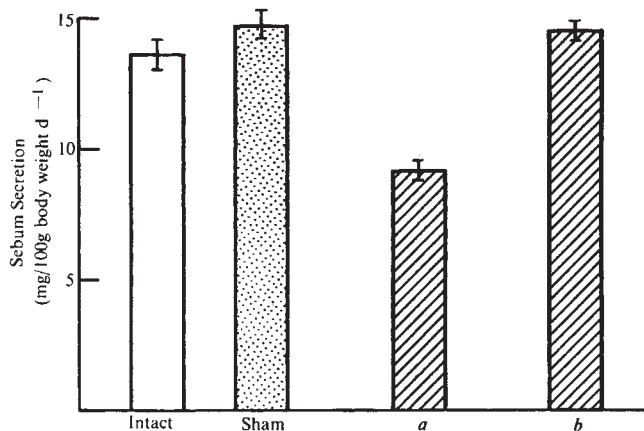
**Table 1** The Rate of Sebum Secretion in Intact and Hypophysectomized Female Rats after Treatment with Synthetic α-MSH, and Hypophysectomized Female Rats after Treatment with Oxytocin ('Syntocinon') and Vasopressin ('Pitressin')

| Treatment                       | No. of rats | Sebum secretion<br>mg d <sup>-1</sup> | P      | mg per 100 g<br>body weight d <sup>-1</sup> | P      |
|---------------------------------|-------------|---------------------------------------|--------|---------------------------------------------|--------|
| (a) Intact                      |             |                                       |        |                                             |        |
| Control                         | 6           | 23.0 ± 0.4                            | —      | 11.1 ± 0.1                                  | —      |
| MSH<br>(50 µg d <sup>-1</sup> ) | 6           | 29.9 ± 1.3                            | <0.001 | 14.6 ± 1.1                                  | <0.001 |
| (b) Hypophysectomized           |             |                                       |        |                                             |        |
| Control                         | 7           | 10.4 ± 0.5                            | —      | 5.5 ± 0.2                                   | —      |
| MSH<br>(50 µg d <sup>-1</sup> ) | 7           | 14.2 ± 0.8                            | <0.005 | 8.1 ± 0.3                                   | <0.001 |
| Control                         | 7           | 10.5 ± 0.4                            | —      | 6.3 ± 0.3                                   | —      |
| 'Syntocinon'                    |             |                                       |        |                                             |        |
| 1.5 U d <sup>-1</sup>           | 5           | 9.7 ± 0.4                             | NS     | 6.0 ± 0.1                                   | NS     |
| 4.5 U d <sup>-1</sup>           | 5           | 10.4 ± 0.4                            | NS     | 6.3 ± 0.3                                   | NS     |
| 'Pitressin'                     |             |                                       |        |                                             |        |
| 1.5 U d <sup>-1</sup>           | 5           | 12.2 ± 0.8                            | NS     | 7.1 ± 0.3                                   | NS     |
| 4.5 U d <sup>-1</sup>           | 5           | 12.8 ± 0.6                            | <0.02  | 7.4 ± 0.02                                  | <0.02  |

Student's *t* test was used to test the significant differences between control and treated groups. NS: not significant at 0.05 level of probability.



**Fig. 2** The sebotropic activity g<sup>-1</sup> pituitary tissue of extracts a, b and c.



**Fig. 3** Effect of removal of the neurointermediate lobe (a) and replacement with α-MSH (30 µg d<sup>-1</sup> for 2 weeks) (b) on the rate of sebum secretion in seven rats compared with seven intact and five sham-operated rats (mean ± s.e.). There was a significant (*P* < 0.001) difference in sebum secretion between the operated rats (a) and the intact controls.

the control of colour change. With the evolution of hair in the mammal, control of skin colour is much less important and the pigmentary hormones seem to have developed new roles. These results suggest that in the hairy mammal MSH may serve as a sebrotrophic hormone concerned with the maintenance of hair lipid.

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## Correlation between Antimicrobial Activity and Peroxidase Content of Leukocytes

MICROBICIDAL assays on polymorphonuclear leukocytes (PMN) have been reported<sup>1-4</sup>, but their application as clinical tests has been delayed, partly because of the relatively large blood volumes required<sup>1-3</sup> and the possible damage to the phagocytosing cells during the separation procedure<sup>3</sup>. Also, accurate determination of the viability of phagocytosed microbes is a problem. Previous methods involved either a disruption of the PMN or indirect criteria for viability such as the incorporation of <sup>3</sup>H-thymidine<sup>4</sup>, the presence of buds on yeast cells<sup>3</sup>, or histochemical integrity of RNA<sup>3</sup>. Our work aimed to develop a direct and sensitive micro-scale method to distinguish small differences in the microbicidal activity of PMN and to evaluate the role of myeloperoxidase (MPO) in this microbicidal function of PMN.

The extent to which MPO is important for the intracellular killing<sup>5</sup> remains questionable.

We have developed a simple and discriminative candidicidal assay and have determined the fungicidal activity of PMN from normal humans, pigs, and chickens. These contain much, little and no MPO respectively (Fig. 1). We also used it in a case of human MPO-deficiency<sup>6</sup> not previously studied. By using these PMN of different MPO-content, we attempted to find a correlation between the MPO and fungicidal activity.

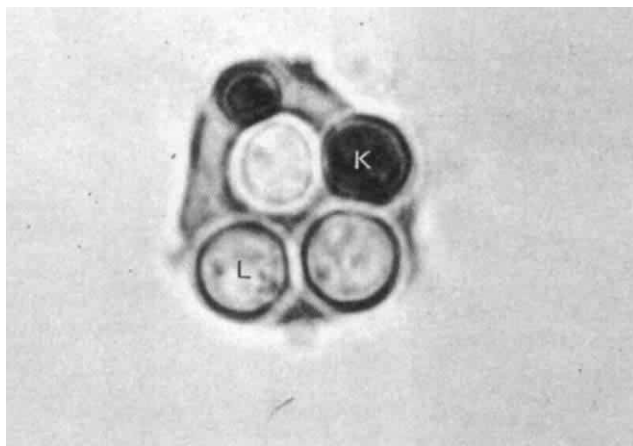


Fig. 2 Photograph of PMN having phagocytosed five CA cells. The killed CA (K) have accumulated methylene blue and appear dark. The living CA (L) have excluded the dye and remain unstained.

Our problem was to demonstrate the viability of *Candida albicans* (CA) inside the secondary lysosomes of the intact PMN. Methylene blue is concentrated in the lysosomes of living phagocytes<sup>7</sup> and, as this only stains non-viable cells, viability was determined using this dye. PMN were obtained by incubating three drops of whole blood on a cover-slide forming the bottom of a moist chamber<sup>8</sup>. After removing the blood-clot the monolayers of PMN were covered by a suspension of CA (2,000 cells  $\mu$ l<sup>-1</sup>) in Hank's solution containing 25% homologous serum. For the last 10 min of the 1 h incubation period, methylene blue (final concentration 2:10<sup>-4</sup> M in Hank's solution) was added to stain the dead phagocytosed CA blue, whereas the living CA remained unstained (Fig. 2). Similar experiments were performed using KCN (final concentration 10<sup>-3</sup> M), an inhibitor of MPO<sup>9</sup>. Experiments using Hank's solution with 25% serum without PMN served as controls.

The advantage of this procedure is that it allows direct microscopic observation of the number of phagocytosed and killed CA. The phagocytosis of CA was accomplished within about 10 min. Means of 4.2 (s.d.  $\pm$  0.33) CCA per PMN were counted for humans, 3.6 (s.d.  $\pm$  0.18) for pigs, and 1.4 (s.d.  $\pm$  0.5) for chickens. The differences are attributed to the difference in size of the PMN. After an incubation period of 1 h the percentage of dead CA in the PMN was determined. The greatest percentage was found in the PMN from humans (Fig. 3). Unlike previous findings with other methods<sup>10</sup> the killing of CA by chicken PMN was significantly lower when compared with human PMN ( $P < 0.0025$ ). Also the efficiency of killing CA was significantly less in pig ( $P < 0.0005$ ) when compared with the normal human. The MPO-deficient human PMN, however, were capable of killing CA to some degree in contrast to other results from an MPO-deficient patient<sup>3</sup>. The experiments using KCN

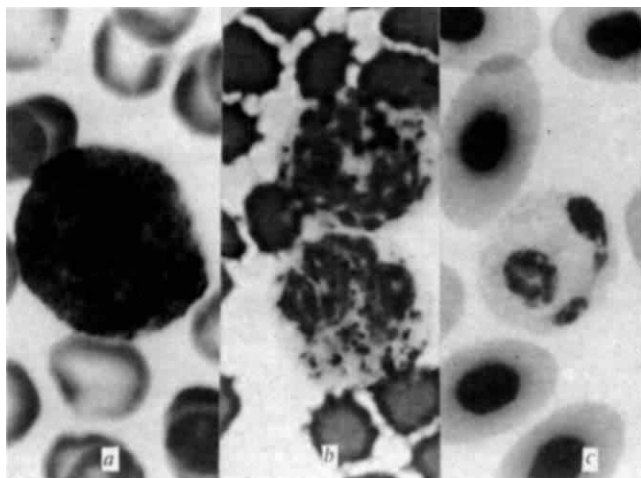


Fig. 1 Blood smears stained for peroxidase (black in photograph) by the method of Graham-Knoll. a, PMN filled with MPO-positive granules from a normal human; b, PMN of pig showing only few MPO-positive granules; c, PMN of chicken completely lacking MPO.



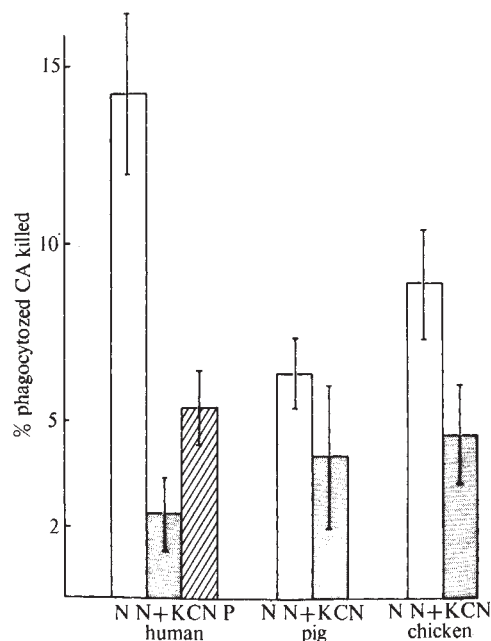


Fig. 3 Percentage of phagocytosed CA killed after 1 h incubation. The CA inside 100 PMN were counted and the dead CA expressed as percentage of the total phagocytosed CA. The values were corrected by subtracting the percentage (<2%) of already dead CA in the suspension before the beginning of the experiment. N, Killing achieved by normal healthy human, pig and chicken PMN incubated with CA suspended in Hank's solution containing 25% serum (mean and s.d. of six duplicate experiments). N+KCN, Killing of CA by PMN incubated as for N but with KCN in a final concentration of  $10^{-3}$  M (mean and s.d. of five duplicate experiments). P, Killing of CA by PMN incubated as for N from an MPO-deficient patient.

showed a reduction in the killing of CA by PMN by 83.4% (s.d.  $\pm 37.4$ ) for normal humans, 37% (s.d.  $\pm 17.1$ ) for pigs, and 48% (s.d.  $\pm 14.3$ ) for the chickens (significant at  $P < 0.05$  respectively). The experiments using only the 25% serum with or without KCN ( $10^{-3}$  M) showed no significant change in the number of dead CA compared with the original suspension (<2%).

Because the PMN of pigs and chickens were less efficient than the human we suggest that MPO may be important for the microbicidal activity. This was supported by experiments with the MPO-inhibitor, KCN, in which killing by the MPO-rich human PMN was reduced much more than that of pigs or chickens.

Species differences in fungicidal activity, however, suggest that the MPO cannot be the only system involved, otherwise we would expect pigs to show a higher fungicidal activity than chickens. Also, the chicken and the MPO-deficient human PMN can kill CA, which suggests that there is another mechanism(s). This would also explain why KCN does not totally prevent the intracellular killing. Such a system could be provided by the basic proteins which are present in large amounts in chicken PMN<sup>11</sup> and only in small amounts in human PMN<sup>12</sup>.

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## Lipid Motion and Rhodopsin Rotation

ROTATIONAL diffusion of rhodopsin in the visual receptor membrane was demonstrated recently by Brown<sup>1</sup>. A relaxation time of about  $\tau \approx 20$   $\mu$ s was measured by Cone<sup>2</sup> for the rotation of rhodopsin about an axis perpendicular to the plane of the membrane. This result was discussed in terms of the viscosity of the lipid matrix of the visual receptor membrane. It was concluded that the lipid matrix is "highly fluid".

On the molecular level the fluidity of a lipid membrane is determined by intra- and intermolecular motions of the lipid hydrocarbon chains and by the rapid lateral displacement of the lipid molecules within the plane of the membrane. By the spin label technique it has been shown that the frequency,  $\nu$ , of this lateral displacement is of the order of  $\nu = 10^7$  s<sup>-1</sup> for biological membranes and model membranes of synthetic lipids at a temperature above their thermal phase transition<sup>3,4</sup>. The corresponding coefficient for the lateral diffusion of the lipid molecules is of the order of  $D_a = 3 \times 10^{-8}$  cm<sup>2</sup> s<sup>-1</sup>. This means that a lipid molecule in a fluid lipid matrix remains at a given site in the lipid lattice for no longer than about  $10^{-7}$  s.

We used a simple model to estimate the rotational relaxation time,  $\tau$ , of rhodopsin from the hopping frequency,  $\nu$ , of the lipid molecules. Figure 1 shows a view from above of a lipid membrane containing a cylindrical macromolecule M embedded between the lipid molecules.  $\lambda$  denotes the average distance between the lipid molecules and is equal to the length of one jump in the lateral motion of the lipid molecules. The rotational axis of M is assumed to be perpendicular to the plane of the membrane. It will be assumed that the cross section of the molecule M is not exactly circular but has at least one "spike", S, of a few Ångströms in height. Then M can rotate only if a vacancy is temporarily created in the neighbourhood of the spike S. This spike may be a local protrusion of the polypeptide chain or it may be a lipid molecule which is firmly attached to the rhodopsin molecule. In this model the rotation of M may be visualized as a sequence of rotational increments each involving an angular change  $\theta_i$ . According to Fig. 1 the value of  $\theta_i$  is related to the average distance,  $\lambda$ , between the lipid molecules by  $\theta_i = \lambda/R$ , where R denotes the radius of the molecule M.

The coefficient of rotational diffusion,  $D_r$ , may then be written as

$$D_r [\text{rad}^2 \text{ s}^{-1}] = 1/2(\nu, \theta_i^2) = 1/2(\nu, \lambda^2 R^{-2})$$

where  $\nu_r (= \nu)$  is the frequency of elementary rotational displacements.

The corresponding value of the rotational correlation time  $\tau$  is given by  $\tau = (2\pi)^2/D_r$ . For a fluid bilayer of dipalmitoyllecithin (at 50° C)  $\nu$  is  $0.9 \times 10^7$  s<sup>-1</sup> (ref. 3). The average distance,  $\lambda$ , between the lipid molecules may be estimated from the average area, F, occupied by a lipid molecule. For a triangular lattice formed by the lipid hydrocarbon chains F is related to the lattice constant a by  $F = a^2 \sqrt{3}$ . A value of  $F = 60$  Å<sup>2</sup> would correspond to  $a = 5.9$  Å. As seen in Fig. 1 the lipid lattice is highly anisotropic; a given lipid molecule has

two nearest neighbours at a distance  $a$ , four neighbours at a distance  $\sqrt{3}a$  and two neighbours at a distance  $2a$ . For  $F=60-80 \text{ \AA}^2$  (ref. 5) we obtain  $a=5.9-6.8 \text{ \AA}$  and an average value of  $\lambda$  ( $=1.62 a$ ) between 9.6 and 11  $\text{\AA}$ .

The exact dimensions and shape of the rhodopsin molecule are not known. In frog rods rhodopsin has a molecular weight of about 40,000 (ref. 6). From an analysis of X-ray intensity data Blasie<sup>7</sup> concluded that the molecules are nearly spherical with a radius of  $R=20-21 \text{ \AA}$ . Wu and Stryer<sup>8</sup> recently measured the energy transfer between fluorescence labels attached to different sites of bovine rhodopsin; their results suggest that rhodopsin has an elongated shape with a length of about 75  $\text{\AA}$ . This would correspond to  $R \approx 11.5 \text{ \AA}$  for a molecular weight of 40,000 and a partial specific volume of about 0.80.

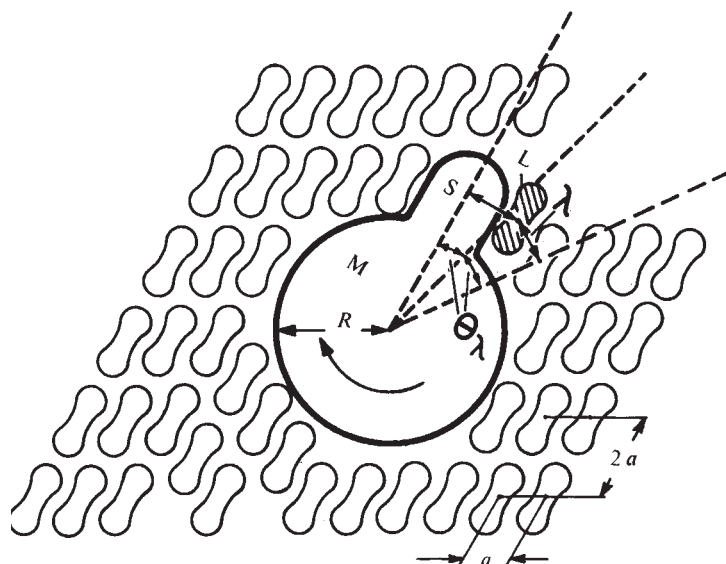


Fig. 1 View from above of a lipid lattice with embedded macro-molecule M. The lipid molecules rapidly diffuse laterally with a hopping frequency  $\nu \approx 10^7 \text{ s}^{-1}$ . Rotation of M about an axis perpendicular to the plane of the membrane by an elementary angle  $\theta_1$  requires that a temporary vacancy is created in the neighbourhood of a protrusion S on M by the lateral displacement of a lipid molecule L. The frequency of the lipid displacements determines the frequency of incremental rotational steps and consequently the rotational correlation time of M. The size of S could actually be much smaller than shown in the drawing.

We estimate the rotational correlation time,  $\tau$ , using the values  $R=15 \text{ \AA}$  and  $R=25 \text{ \AA}$ . Somewhat larger values for  $R$  are used because it is the distance between the protrusion, S, and the centre of the molecule that determines the incremental rotational angle  $\theta_1$ . For  $R=15 \text{ \AA}$ ,  $\lambda=10 \text{ \AA}$  and  $\nu=10^7 \text{ s}^{-1}$  we obtain  $D_r=2.22 \times 10^6 \text{ rad}^2 \text{ s}^{-1}$  or  $\tau=(2\pi)^2 D_r^{-1}=18 \text{ \mu s}$ . Using  $R=25 \text{ \AA}$ ,  $\lambda=10 \text{ \AA}$  and  $\nu=10^7 \text{ s}^{-1}$  we arrive at  $D_r=0.8 \times 10^6 \text{ rad}^2 \text{ s}^{-1}$  or  $\tau=50 \text{ \mu s}$ .

These results suggest that the hopping frequency of the lipid molecules in the visual receptor membrane is about equal to, or is two to three times larger than, the hopping frequency in dipalmitoyl-lecithin bilayers at  $50^\circ \text{C}$ . This seems plausible because the lipid molecules in the visual receptor membrane contain highly unsaturated hydrocarbon chains<sup>9</sup>.

Our results remain virtually unchanged if the molecule M has two (or more) spikes so that one rotational step requires the simultaneous displacement of two (or more) lipid molecules. This is because the relaxation time for the free rotation of a molecule M with a molecular weight of 40,000 is of the order of  $10^{-9} \text{ s}$  in the absence of environmental restrictions as estimated from the kinetic energy of rotation at 293 K. This value is very small compared with the lifetime of a vacancy in the lipid lattice which may be assumed to be equal to the inverse

of the jump frequency: about  $10^{-7} \text{ s}$ . Therefore, if two lipid molecules at different sites must be displaced in order to allow one rotational step ( $\theta_1$ ) these two places need be vacant simultaneously only for about  $10^{-9} \text{ sec}$ . For a lifetime of a vacancy of about  $10^{-7} \text{ s}$  the probability of this happening is almost unity.

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## Orotic Aciduria and Arginine Deficiency

GROWING rats<sup>1</sup>, guinea-pigs<sup>2</sup>, rabbits<sup>3</sup> and possibly human beings<sup>4</sup> require dietary arginine for optimum nitrogen retention and normal growth. Adult men<sup>5</sup>, dogs<sup>6</sup> and rats<sup>1</sup> do not require dietary arginine for nitrogen equilibrium and it is assumed that adults of other species also meet all of their arginine needs from synthesis within tissues. Kennan and Cohen<sup>7</sup> have suggested, however, that the Krebs-Henseleit urea cycle, in which arginine is an intermediate, operates at near maximum capacity for ammonia (that is  $\text{NH}_3 + \text{NH}_4^+$ ) detoxification in immature rats. Sperm, which contain a large concentration of arginine, decrease in numbers and motility when collected from mature men or rats fed arginine-deficient diets<sup>8,9</sup>. Free orotic acid found normally in milk<sup>10</sup> cannot be readily detected in normal tissues<sup>11</sup>. It is barely detectable in normal urine<sup>12</sup> unless orotate is fed (our unpublished results). We have observed profound orotic aciduria in arginine-deficient growing rats<sup>13</sup> and in ammonia-intoxicated rats (unpublished results). The accumulated evidence suggested that endogenous synthesis provides limited reserves of arginine for detoxifying ammonia in mature animals and that important metabolic consequences might ensue if their diet lacked arginine. We report here experiments with rats which clearly show that marked increases in orotate and urea excretion are sensitive indicators of dietary arginine deficiency, regardless of age and body size. The data also show that the usual methods of determining amino acid needs for adult mammals fail to reveal biochemical changes caused by diets lacking available arginine.

Sprague-Dawley rats reared on a commercial diet fed *ad lib.* were placed on an L-amino acid diet (control)<sup>14</sup> when they weighed 40, 175 or 500 g. After 3 d, half the rats were switched to the same diet devoid of arginine ( $-\text{Arg}$ ). The nitrogen content of the  $-\text{Arg}$  diet was equalized to the control with glycine. Urine was collected under acidified toluene. Portal and peripheral venous blood was collected under anaesthesia<sup>15</sup>. Orotate<sup>16</sup>, urea<sup>17</sup> and ammonia<sup>18</sup> were determined colorimetrically. The 24 h orotic acid excretion for 40, 150 and 500 g rats fed the complete diet averaged  $13 \pm 1$ ,  $8 \pm 5$ , and  $16 \pm 4 \text{ \mu g}$ , respectively. During the first

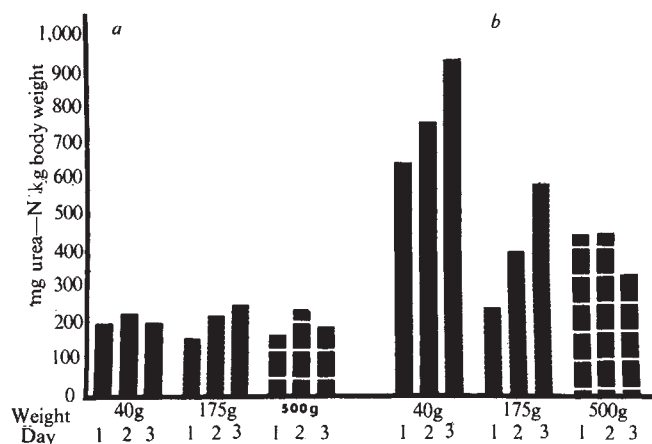
24 h on the —Arg diet, orotic acid excretion reached 225, 150 and 25 times control levels for 40, 150 and 500 g rats, respectively. After 72 h the corresponding multiples were 300, 360 and 175 (Fig. 1). Orotate excretion was greatest for 40 g rats, intermediate for 175 g rats and lowest in 500 g rats. All —Arg rats excreted significantly more orotate than their respective controls. Growing puppies also showed profound increases in urinary orotate within 24 h after they began consuming an arginine deficient diet (our unpublished results).

**Table 1** Blood Urea and Ammonia in Rats Fed an Amino Acid Mixture with and without Arginine

|                                                             | Control               | —Arginine             |
|-------------------------------------------------------------|-----------------------|-----------------------|
| Peripheral $\text{NH}_3\text{-N}$ ( $\mu\text{g ml}^{-1}$ ) | $2.5 \pm 0.1$         | $4.0 \dagger \pm 0.5$ |
| Portal $\text{NH}_3\text{-N}$ ( $\mu\text{g ml}^{-1}$ )     | $3.7 \dagger \pm 0.2$ | $7.1 \dagger \pm 0.2$ |
| Portal/peripheral                                           | 1.5                   | 1.8                   |
| Peripheral urea-N ( $\mu\text{g ml}^{-1}$ )                 | $7.6 \pm 1.0$         | $11.8 \pm 0.6$        |
| Portal urea-N ( $\mu\text{g ml}^{-1}$ )                     | $9.7 \pm 0.6$         | $13.0 \pm 0.8$        |
| Portal/peripheral                                           | 1.3                   | 1.1                   |

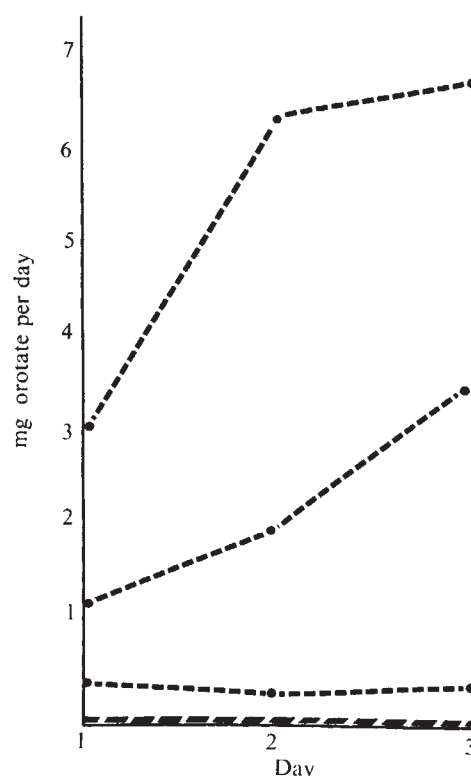
Horizontal means with unlike superscripts differ,  $P < 0.05$ .

Urinary urea excretion also increased during the first 24 h (Fig. 2) of —Arg feeding and the difference relative to control increased with time in the 40 and 175 g rats. Portal and peripheral blood ammonia concentrations were significantly greater 2 h after feeding in rats given the —Arg diet (Table 1). We attribute the increased blood ammonia to enhanced amino acid catabolism<sup>19,20</sup> due to amino acid imbalance, and to increased urea hydrolysis<sup>21</sup> in the gastrointestinal tract. As previously, blood urea was also significantly greater in rats given the —Arg diet<sup>22</sup>. The large concentrations of blood and urinary urea reflect increased detoxification of ammonia by the liver.



**Fig. 1** Urinary urea excretion by rats fed a purified amino acid diet with (a) or without (b) arginine.

The profound increase in orotate excretion is undoubtedly due to shunting of carbamyl phosphate into pyrimidine biosynthesis. The significant increases seen in urinary orotic acid during hyperammonaemia<sup>12</sup>, altered pyrimidine metabolism<sup>23</sup> and arginine deficiency<sup>13</sup> support this conclusion. The ability of arginine to protect ammonia-intoxicated animals emphasizes its role in the metabolic handling of ammonia<sup>12,24</sup>. Incorporation of ammonia into carbamyl phosphate is the initial step in arginine formation<sup>25,26</sup> by way of the urea cycle and likewise in the production of pyrimidine components<sup>27,28</sup> for DNA and RNA through the orotate pathway.



**Fig. 2** Urinary orotate excretion by rats fed a purified amino acid diet with or without arginine. —, Control. ---, Arginine-deficient: top line, 40 g rats; middle line, 175 g rats; bottom line, 500 g rats.

Our studies indicate that profound increases in urinary excretion of endogenously formed orotic acid do not occur with other amino acid deficiencies<sup>21</sup>. When single or combinations of amino acids are introduced into the body in excess, deamination causes a transient increase in tissue fluid ammonia, perhaps exceeding the capacity of the liver for urea synthesis. This can occur without peripheral blood ammonia concentrations reaching toxic levels<sup>19,20</sup>. Increased orotic acid excretion probably occurs in such circumstances.

Our results show that metabolic pathways of growing and mature animals respond rapidly to arginine deficiency by changing the excretion of primary end products of nitrogen metabolism. Arginine deficient animals appear to provide a valuable model for studying the control of pyrimidine biosynthesis. Organic acids of the tricarboxylic acid cycle also increase significantly in urine<sup>13</sup>. In view of the similarities in intermediary metabolism in animals, our findings demonstrate the need for comparable studies in other ureotelic species, including man.

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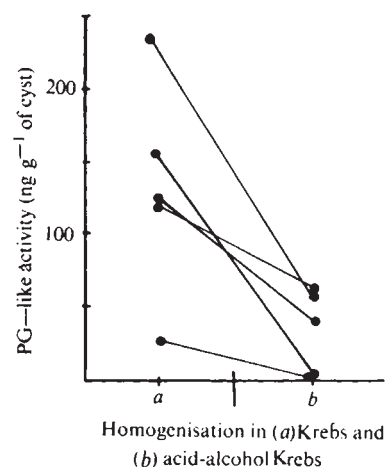


Fig. 1 Prostaglandin-like activity in homogenates of dental cyst (a) in Krebs solution, and (b) after enzyme inactivation in acid-ethanol Krebs solution.

Samples were also assayed for parathyroid hormone using a radioimmuno technique, but none was found.

The media of cysts without indomethacin contained high levels of PG-like activity ( $1.7\text{--}81\text{ }\mu\text{g g}^{-1}$  cyst, mean  $\pm$  s.e.m.  $19 \pm 7.0$  assayed as  $\text{PGE}_2$ ) (Table 1). Much smaller amounts ( $0.0007\text{--}1.0\text{ }\mu\text{g g}^{-1}$  cyst, mean  $\pm$  s.e.m.  $0.2 \pm 0.1$ ) were extracted from the media containing indomethacin ( $P < 0.02$ ). The seven periapical cysts which form at the apices of non-vital teeth released more PG-like material ( $5.6\text{--}81\text{ }\mu\text{g g}^{-1}$  cyst as  $\text{PGE}_2$ , mean  $\pm$  s.e.m.  $28 \pm 9.6$ ) than the four developmental follicular cysts ( $1.7\text{--}6.0\text{ }\mu\text{g g}^{-1}$ , mean  $\pm$  s.e.m.  $3.4 \pm 1.0$ ,  $P < 0.05$ ).

Table 1 Prostaglandin-like Material Produced by Dental Cysts

| No. | Cyst type | Prostaglandin-like material (as $\text{PGE}_2$ ) released in cyst 48 h culture media |                                                                   |                     |                           |
|-----|-----------|--------------------------------------------------------------------------------------|-------------------------------------------------------------------|---------------------|---------------------------|
|     |           | Reynolds BGJ medium                                                                  | Reynolds BGJ plus indomethacin ( $5\text{ }\mu\text{g ml}^{-1}$ ) |                     |                           |
|     |           | ng $\text{ml}^{-1}$                                                                  | $\mu\text{g g}^{-1}$ cyst                                         | ng $\text{ml}^{-1}$ | $\mu\text{g g}^{-1}$ cyst |
| 1   | P*        | 1,820                                                                                | 81                                                                | 10                  | 0.4                       |
| 2   | P         | 490                                                                                  | 25                                                                | 0.13                | 0.1                       |
| 3   | P         | 355                                                                                  | 15                                                                | 2.9                 | 0.1                       |
| 4   | P         | 190                                                                                  | 5.6                                                               | 0.18                | 0.006                     |
| 5   | P         | 185                                                                                  | 39                                                                | 4.0                 | 1.0                       |
| 6   | P         | 520                                                                                  | 17                                                                | 5.0                 | 0.2                       |
| 7   | P         | 480                                                                                  | 15                                                                | <0.02               | <0.0007                   |
| 8   | F†        | 46                                                                                   | 1.7                                                               | 5.0                 | 0.2                       |
| 9   | F         | 240                                                                                  | 6.0                                                               | 3.0                 | 0.1                       |
| 10  | F         | 97                                                                                   | 3.4                                                               | <0.02               | <0.0007                   |
| 11  | F         | 91                                                                                   | 2.5                                                               | 8.0                 | 0.4                       |

\* Inflammatory periapical cyst. † Developmental follicular cyst.

## Prostaglandin Production and Bone Resorption by Dental Cysts

SEVERAL malignant tumours<sup>1-4</sup> and benign dental cysts<sup>5</sup> resorb bone in tissue culture. As prostaglandins can resorb bone<sup>6</sup> and the mouse fibrosarcoma synthesizes  $\text{PGE}_2$ -like material<sup>7</sup>, we studied the possibility that the resorption factor produced by dental cysts is a prostaglandin.

Dental cysts were carefully enucleated from anaesthetized patients and quenched in hexane and dry ice. Weighed samples were tested for their content and ability to synthesize prostaglandin-like material by homogenizing for 30 s (Silverson homogenizer) in either Krebs solution alone, or Krebs solution and ethanol (1:1) acidified to about pH 3 with formic acid to inhibit prostaglandin synthesis. The homogenates were extracted<sup>8</sup> and assayed on the rat fundus strip<sup>9</sup> in Krebs solution at 37° containing hyoscine, methysergide, mepyramine (all  $0.2\text{ }\mu\text{g ml}^{-1}$ ), 'Hydergine' (Sandoz) ( $0.25\text{ }\mu\text{g ml}^{-1}$ ) prone-thalol ( $1\text{ }\mu\text{g ml}^{-1}$ ) and indomethacin ( $1\text{ }\mu\text{g ml}^{-1}$ ). These drugs increase the selectivity and often the sensitivity of the assay.

Extracts of cyst homogenized in Krebs solution alone contained substantial amounts of prostaglandin-like activity ( $23\text{--}233\text{ ng g}^{-1}$  as  $\text{PGE}_2$ ) whereas acidified ethanol homogenates contained smaller 'basal levels' ( $0\text{--}60\text{ ng g}^{-1}$  as  $\text{PGE}_2$ ; five experiments). The mean reduction (89%) was significant ( $P < 0.001$ ;  $t = 8.74$ ) (Fig. 1). Cyst tissue therefore appeared to form prostaglandin-like material when enzyme activity was not inhibited<sup>10</sup>.

Cysts were maintained in Hanks solution at 0° C for a maximum of 1 h after enucleation. Full-thickness samples, 1-2 mm square, were cultured for 48 h in Reynolds modified BGJ medium<sup>11</sup> gassed with 5%  $\text{CO}_2$  in air, alone or with indomethacin  $5\text{ }\mu\text{g ml}^{-1}$  to inhibit prostaglandin synthesis<sup>12</sup>. Aliquots of each medium were extracted, bio-assayed for prostaglandins, and chromatographed in the AI and AII solvent systems<sup>13</sup> as modified by Stamford and Unger<sup>14</sup>.

Most of the prostaglandin-like material ran at the same  $R_f$  (0.62-0.65) as PGE compounds using the system to separate type E from type F compounds<sup>14</sup>, but in seven out of thirteen experiments some ran at the  $R_f$  of PGF compounds (0.47-0.55). PGE alone was detected in six experiments, PGE and F were indicated in six, and PGF-like activity alone in one experiment. Alkaline hydrolysis ( $0.2\text{ N NaOH}$ , 30 min, 37° C), which inactivates PGE but not PGF compounds, inactivated the material running at the PGE  $R_f$  value but not at the PGF  $R_f$  value (three experiments). In fourteen cysts chromatography in the AII system<sup>13</sup> tentatively indicated varying proportions of the following;  $\text{PGE}_1$  ( $R_f$  0.77-0.88; six cysts),  $\text{PGE}_2$  ( $R_f$  0.57-0.67; thirteen cysts),  $\text{PGF}_1$  ( $R_f$  0.57-0.67; five cysts),  $\text{PGF}_2$  ( $R_f$  0.31-0.48; eight cysts), and a slower running material with an  $R_f$  value similar to that of  $\text{PGE}_3/\text{PGF}_3$  (0.15-0.23; nine cysts). In five cyst incubates  $\text{PGE}_2$ -like material was present

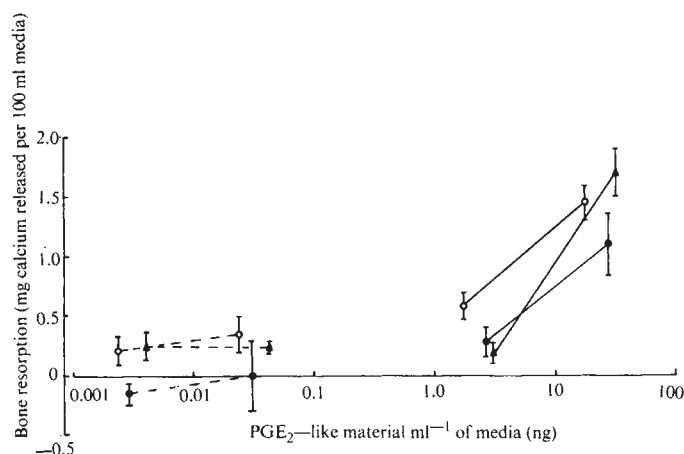


Fig. 2 Bone resorption produced by 1 in 30 and 1 in 300 dilutions of cyst culture media without indomethacin (—) compared with the indomethacin (---) containing media showing diminished bone resorption and release of prostaglandin-like material (measured as  $E_2$ ).

in the greatest amount ( $1.3\text{--}12.2 \mu\text{g g}^{-1}$  assayed as  $E_2$ ) and in six other cysts the slower running material was the most abundant ( $1.7\text{--}33 \mu\text{g g}^{-1}$  assayed as  $E_2$ ),  $\text{PGF}_2$  was predominant in two cysts and  $\text{PGE}_1$  in one. If the slower running material is  $\text{PGE}_3$  or  $\text{PGF}_3$ , the amounts would be considerably greater than those stated in terms of  $\text{PGE}_2$  as the rat fundus strip is approximately ten times less sensitive to  $\text{PGE}_3$  and 100 times less sensitive to  $\text{PGF}_3$  than to  $\text{PGE}_2$ .

Table 2 Bone Resorption by Cyst Culture Media

| Cyst | PG content<br>$\text{ng ml}^{-1}$<br>media<br>as $E_2$ | Bone resorption* (net mg Ca released per 100 ml media) |                                                             |                                                                    |
|------|--------------------------------------------------------|--------------------------------------------------------|-------------------------------------------------------------|--------------------------------------------------------------------|
|      |                                                        | (1)<br>Cyst media †                                    | (2)<br>Cyst media<br>+ PPP<br>( $100 \mu\text{g ml}^{-1}$ ) | (3)<br>Cyst media +<br>indomethacin<br>( $5 \mu\text{g ml}^{-1}$ ) |
| 1    | 607                                                    | $1.60 \pm 0.47$                                        | $0.30 \pm 0.39$                                             | $1.65 \pm 0.25$                                                    |
| 2    | 162                                                    | $1.22 \pm 0.09^\ddagger$                               | $0.12 \pm 0.2$                                              | $1.32 \pm 0.22$                                                    |
| 3    | 118                                                    | $2.52 \pm 1.01$                                        | —                                                           | —                                                                  |
| 4    | 63                                                     | $3.39 \pm 0.20^\ddagger$                               | $1.29 \pm 0.23$                                             | $2.24 \pm 0.63$                                                    |
| 5    | 62                                                     | $0.73 \pm 0.11^\S$                                     | $0.03 \pm 0.17$                                             | $1.03 \pm 0.16$                                                    |
| 6    | 173                                                    | $2.66 \pm 0.31^\ddagger$                               | $0.31 \pm 0.19$                                             | $2.55 \pm 0.24$                                                    |

\* Resorption is expressed as the difference in calcium release between the experimental and control halves of each calvarium. Mean  $\pm$  s.e.m. of four calvaria.

† Cyst culture media diluted 1 : 2.

‡ Highly significant ( $P < 0.001$ ) difference between groups (1) and (2).

§ Significant ( $P < 0.01$ ) difference between groups (1) and (2).

As gingiva also releases bone resorbing material<sup>3,15</sup> we compared the relative abilities of cyst and gingiva to produce prostaglandins. Specimens of clinically uninfamed gingiva were cultured with the addition of nystatin,  $6 \mu\text{g ml}^{-1}$  media, to prevent monilial overgrowth. The range of prostaglandin-like material detected after 48 h culture in plain media was  $1.5\text{--}6.8 \mu\text{g g}^{-1}$  gingiva measured as  $E_2$  (mean  $\pm$  s.e.m.  $3.3 \pm 0.5$ , twelve experiments). This closely resembled the follicular cyst range, and was significantly less ( $P < 0.005$ ) than the periapical cysts. Chromatography in the AII solvent system of eleven samples of gingiva indicated mixtures of prostaglandin-like material with an  $R_f$  of  $\text{PGE}_1$  (seven samples),  $\text{PGE}_2$  (ten samples),  $\text{PGF}_1$  (six samples),  $\text{PGF}_2$  (eight samples) and  $\text{PGE}_3/\text{F}_3$  (three samples). The predominant prostaglandins were  $\text{PGE}_2$ -like material in eight cases,  $\text{PGE}_1$ -like material

in two cases and  $\text{PGF}_3$ -like material in one case. A breach in the periosteum in early periodontal disease might lead to bone destruction by this prostaglandin-like material.

Samples of cyst culture media without indomethacin were added as described below to half calvaria (six-day-old albino mice) previously cultured in Reynolds BGJ medium for 48 h. Each calvarium was initially halved along the sagittal suture to provide experimental and control bones. The experimental bone was cultured in 1.5 ml of cyst culture medium diluted 1:2 with fresh medium alone (Group 1) or containing the prostaglandin antagonist polyphlorethin phosphate (PPP)<sup>16,17</sup>  $100 \mu\text{g ml}^{-1}$  (Group 2), or indomethacin  $5 \mu\text{g ml}^{-1}$  (Group 3). Each group consisted of four calvaria, the controls containing the inhibitors in fresh media. The cultures were gassed with 5%  $\text{CO}_2$  in air and incubated at  $37^\circ \text{C}$  for a further 60 h. Resorption was estimated by atomic absorption spectrophotometry as the increased release of calcium by the experimental bones. The results were calculated using Zanelli's correction formula<sup>18</sup>.

The cyst culture media caused substantial bone resorption significantly inhibited by the prostaglandin antagonist PPP (Table 2) but there was no inhibition when indomethacin was added (Group 3). Bone resorption still occurred when the plain culture media were diluted in 1 in 30 and 1 in 300, but not with the comparable indomethacin culture media (Fig. 2). Indomethacin thus reduces both the release of prostaglandin-like material by the cyst and the production of the bone resorbing factor. Comparison of the dilution/resorption curves for a cyst culture medium (assayed as  $E_2$ ) and standard preparations of  $\text{PGE}_2$ ,  $\text{F}_1$  and  $\text{F}_3$  also suggests that the resorption by the cyst media is due to prostaglandins (Fig. 3). As the rat fundus is relatively insensitive to  $\text{PGF}_1$  and  $\text{PGF}_3$ , their concentrations in the cyst media will be underestimated.

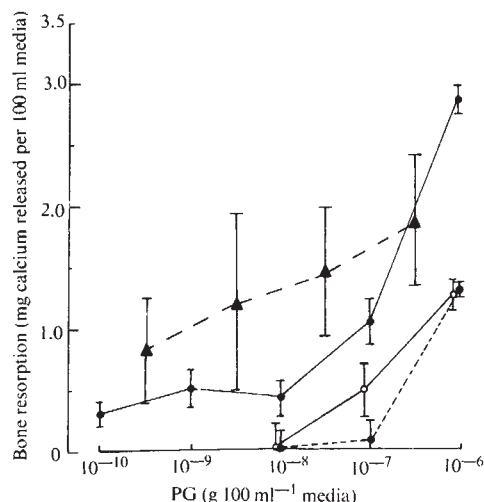


Fig. 3 A dilution/resorption curve for a cyst culture medium (—●—●), compared with those of standard preparations of  $\text{PGE}_2$  (—●—●),  $\text{PGF}_{1a}$  (---●---) and  $\text{PGF}_{3a}$  (---○---).

If prostaglandins play a role in bone resorption by dental cysts they must presumably be released at a site accessible to the bone. Bone resorption is not significantly less when the calvaria are incubated with cyst capsule rather than with full-thickness cyst wall<sup>5</sup>. The bone-resorbing factors therefore seem to be released by the outer capsule adjacent to the bone rather than by the lining epithelium. These observations support the possibility that prostaglandins (perhaps mainly  $\text{PGE}_2$ ) produced by dental cyst capsule are responsible for bone resorption by cysts within the jaws.

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## Growth Hormone and Premature Ageing

It has often been suggested that the immune system may be involved in ageing<sup>1-4</sup>. Fabris *et al.*<sup>5</sup> claim that such immune deficiencies are caused by inadequate stimulation of the lymphoid system by growth hormone (GH). They compared mice with inherited pituitary dwarfism (homozygotes for Snell's dwarf mutation, *dw*) with normal mice from the same stock. The life span of the dwarfs was only 3 to 5.5 months (mean 4.5 months) although the mean life span of their normal sibs was 20 months. The early death of the mutants was taken as a sign of premature ageing. The loss and greying of the hair, cutaneous atrophy, and bilateral cataracts which occurred in the dwarf mice from the age of 2 to 3 months were also taken as signs of ageing, as such changes only occurred in normal littermates during their last few months of life. The uptake of <sup>3</sup>H-thymidine by several tissues, including the thymus, of a 5-month-old dwarf was less than by the corresponding tissues of a 17-month-old normal mouse. The uptakes were greater in both dwarf and normal mice aged 1 month.

The immune system was considered to play a role in these ageing processes for two reasons. First, these dwarf mice had lower thymus and spleen weights, and deficiencies of immune response when compared with normal mice<sup>6</sup>. Second, their life span could be prolonged by giving a single injection of lymphocytes from a normal mouse<sup>5</sup>. The chief cause of the defects in the immune system was considered to be a lack of pituitary GH, because this hormone is lacking in dwarf mice<sup>7</sup> and because the life span of the dwarfs could be extended to a year by giving them thirty daily injections of GH and thyroxine<sup>8</sup>. If these causal relations are general, premature ageing should be a consistent feature of syndromes of growth hormone deficiency.

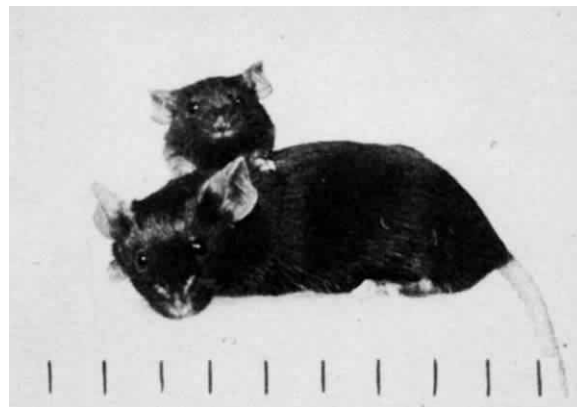


Fig. 1 Two female littermates aged 5 months. One is dwarf (*dw dw*) and the other is normal. Both mice have black, non-agouti, fur. The scale divisions are cm.

I have studied a stock<sup>8</sup> of mice segregating for the same pituitary dwarfism mutation (*dw*). Figure 1 shows two female littermates aged 5 months. One of them is a dwarf. Both mice are alive and have full complements of hair. A few white hairs can be detected in the coats of both mice. This is normal for mice with black, non-agouti, fur<sup>9</sup>. Figure 2 shows the same 5-month-old female dwarf accompanied by a 5-month-old male dwarf. The fur of both is normal, as it is in other dwarfs of this stock at this age. Cataracts are absent from these mice.

The dwarf mice in our stock, which is maintained under conventional conditions, are larger than those of Fabris *et al.*<sup>5</sup> (10-13 g as against 7 g) and live longer. The mice in the photographs, and other dwarf mice born at the same time, have now reached the age of 8 months and still show no signs of premature ageing. At the age of 4 months the relative weight of the thymus was similar in dwarfs and their normal littermates ( $187 \pm 16$  and  $190 \pm 23$  mg per 100 g body wt). The spleens, however, were relatively smaller in the dwarfs ( $106 \pm 18$  as against  $273 \pm 73$  mg per 100 g). The corresponding values for the mice of Fabris *et al.*<sup>5</sup> at the age of 3 months were: thymus, 90 and 100 mg per 100 g, and spleen, 270 and 500 mg per 100 g.

I have confirmed, using polyacrylamide gel electrophoresis<sup>7</sup>, that GH and prolactin are absent from the pituitaries of our dwarf mice. Thus, although hormone deficiencies may play a part, other factors present in the stock of Fabris *et al.* must be of major importance in determining the occurrence of thymic deficiencies and signs of premature ageing. These factors could be genetic, as the genetic backgrounds of the two stocks are different<sup>8</sup> and the existence of genes which modify the expression of major mutants is well established<sup>10</sup>.

Premature ageing is not a characteristic of the other pituitary dwarf mouse (refs 11 and 12 and my personal observation) (Ames dwarf, *df*), nor of the several inherited growth hormone deficiencies in man<sup>13</sup>. Several of the variants caus-



ing dwarfing in man result in wrinkled skin, but such skin is very different from the atrophied skin resulting from normal ageing processes<sup>13</sup>. Pygmies, who have a genetic resistance to GH, do not show premature ageing<sup>13</sup>.



Fig. 2 The dwarf mouse at the front is female and has black, non-agouti, fur. The second mouse is also a dwarf, but is a male with black, agouti, fur. Both mice are 5 months old.

The assumption that a deficiency of GH alone is the cause of premature ageing in the dwarf mice of Fabris *et al.* stock takes no account of the evidence that pituitary dwarf mice also lack prolactin<sup>7,14</sup> and are hypothyroid<sup>15,16</sup>. The equation of shortened life span with premature ageing may not be justified, as the resistance to infection of the immunologically deficient dwarfs may be considerably reduced. In such circumstances the altered uptake of thymidine in dwarf mice close to death could be caused by stress rather than by premature ageing, for the permanent cortex of the adrenal gland is not deficient in dwarf mice<sup>8</sup>.

Although GH deficiency could be important in the premature ageing of individuals of particular genotypes, its role as a major factor in the initiation of ageing is far from proven.

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## Regional and Historical Aspects of Lead Pollution in Britain

Two major periods of increase in atmospheric lead concentrations have been recognized in the Northern Hemisphere—the first resulting from the rapid growth of industry during the eighteenth and nineteenth centuries, the second from the great expansion of motor vehicle traffic since the end of the Second World War<sup>1,2</sup>. There is clear evidence of lead pollution caused by motor vehicles close to busy roads<sup>2-4</sup>, and the three-fold increase in lead concentrations in snow from the Greenland ice-cap during the past 20 yr has also been correlated with the emission of lead aerosols from car exhausts<sup>1</sup>. In Britain many minerals, including lead, have been exploited over a long period of time, but some, including lead, have declined in importance in the past 100 yr after peak extraction levels in the eighteenth and nineteenth centuries. Thus it is probable that in certain areas of Britain heavy metal pollution has been worse in the past than it is now. This communication examines the historical trends in lead pollution in Britain by use of nineteenth century and present-day moss specimens, and by peat profiles—methods used also by other workers to demonstrate historical trends in heavy metal pollution in Scandinavia<sup>2,5</sup>.

Table 1 Lead Contents of Nineteenth Century and Present-day Specimens of *Hypnum cupressiforme*

|                | Mean lead content (p.p.m.) |            | Degrees of freedom | <i>t</i> |
|----------------|----------------------------|------------|--------------------|----------|
|                | 19th century               | 1972-73    |                    |          |
| Snowdonia      | 99.4 (10)                  | 83.6 (12)  | 20                 | 0.649    |
| Morecambe Bay  | 72.2 (8)                   | 73.9 (9)   | 15                 | 0.105    |
| Derbyshire     | 208.2 (16)                 | 222.5 (10) | 24                 | 1.210    |
| Teesdale       | 215.6 (10)                 | 71.2 (10)  | 13                 | 3.806*   |
| North Scotland | 28.6 (10)                  | 18.6 (19)  | 23                 | 1.832    |
| West Ireland   | 31.5 (8)                   | —          | —                  | —        |

The figures in parentheses are the numbers of samples analysed.

Morecambe Bay samples are from Carboniferous limestone outcrops around the Bay. Derbyshire samples are all from the White Peak, and the majority are from the Buxton area. Teesdale samples are from upstream of Middleton-in-Teesdale. North Scotland samples are from the south shore of the Moray Firth between Banff and Inverness, and from Wester Ross. Irish samples are from the coasts of Counties Sligo, Galway, and Clare. Five of the Snowdonia samples are from the Conway Valley, the rest are scattered through the region.

\*  $P=0.01-0.001$ . Because of unequal variances, the degrees of freedom have been adjusted in some cases.

The Manchester Museum contains good collections of many species for the period 1850-1900, including the widespread moss *Hypnum cupressiforme*, which was used by Rühling and Tyler in their study of pollution in Scandinavia<sup>2</sup>. Herbarium specimens of *Hypnum cupressiforme* from six areas in Britain were selected for our study. Most specimens dated from the period immediately following the decline of lead mining in Britain around 1870, but there were earlier specimens from Teesdale covering the period 1850-1870 (when the mines were at peak production). Present-day specimens were collected during 1972 and 1973 from sites as close as possible to those of the herbarium gatherings, but always at a distance of at least 100 m from the nearest road. Before analysis all moss samples were washed in several changes of de-ionized water to remove superficial dust and soil particles, and then dried in an oven at 80° C. The dried samples were ground with a pestle and mortar, and digested in a nitric-perchloric acid mixture (4:1 volume). The resulting solutions were analysed for lead (and also other heavy metals) by atomic absorption spectrophotometry.

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The results of the analyses are shown in Table 1. Clear regional differences are apparent, both for the herbarium and the present-day specimens, with consistently low lead contents recorded for oceanic areas of Britain remote from large centres of population (North Scotland and West Ireland, < 32 p.p.m.). The highest nineteenth century values (> 200 p.p.m.) are from the old lead mining and smelting areas of Teesdale and the White Peak of Derbyshire. The levels from present-day Teesdale samples are significantly lower, and presumably relate to the demise of the lead industry there after 1870. In the Derbyshire samples, however, present-day lead concentrations remain high, and it is possible that these reflect the reworking of the tips for fluorspar and the close proximity of other industries. Intermediate lead contents (70–100 p.p.m.) are recorded from both nineteenth century and present-day specimens from Snowdonia and the Morecambe Bay region. Lead was formerly mined in Snowdonia in considerable quantities (though little was smelted there), while other nineteenth century extractive industries (notably iron) around Morecambe Bay may have contributed indirectly to the increased lead concentrations there. In none of the regions is there any significant increase in the lead content of the present-day moss samples, so that the data do not support Rühling and Tyler's idea of a second increase in lead pollution in recent decades. What cannot be determined from the moss data is the overall pattern of lead values during the past 100 yr, since herbarium specimens from the past few decades, unless collected from precisely documented sites, are useless because of the possibility of lead contamination from motor vehicles.

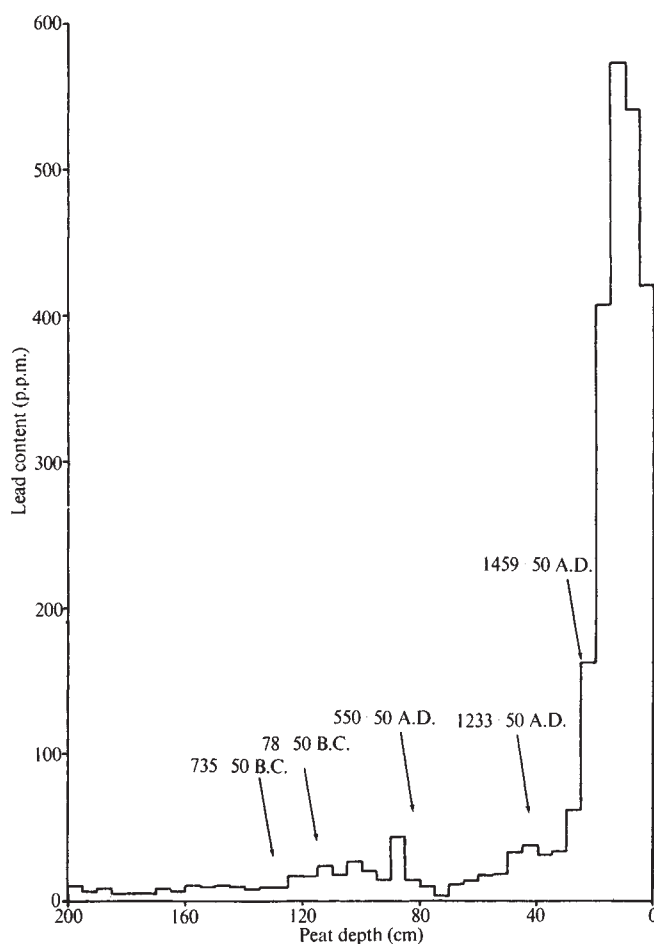
**Table 2** Summary of the Evidence Used for Dating the Buxton Pollen Diagram

| Depth (cm) | Feature in pollen diagram              | Interpretation                                                                   | Suggested dating (AD) |
|------------|----------------------------------------|----------------------------------------------------------------------------------|-----------------------|
| 17–18      | Appreciable cereal pollen values       | Cereals grown in Derbyshire by 1750 (ref. 7)                                     | Post-1650             |
| 9–10       | Increase in pine pollen values         | Early tree planting around Buxton <sup>8</sup>                                   | Post-1790             |
| 8–9        | First detectable soot in peat          | Early industrialization                                                          | 1780–1800             |
| 7–8        | Lowered cereal pollen values           | Post-Napoleonic War depression <sup>7,9</sup>                                    | 1815–1830             |
| 6–7        | Marked increase in ash pollen          | Oldest ash trees in Derbyshire mostly 140–160 yr old <sup>12</sup>               | 1830–1850             |
| 5–6        | Marked increase in soot levels in peat | Expansion of Buxton after 1851 (ref. 10)                                         | Post-1850             |
| 3–5        | Lowered cereal pollen values           | Agricultural depression in Derbyshire at end of nineteenth century <sup>11</sup> | 1880–1905             |
| 2–3        | Marked rise in elm pollen              | Most elm trees in Derbyshire 70 yr old or less <sup>12</sup>                     | 1920–1930             |

Peat deposits, on the other hand, contain a continuous record of lead deposition and uptake by the bog vegetation over long periods of time. The pattern of lead values shown in a peat profile could, however, be modified by differences in uptake by different types of bog vegetation, and by downwashing of material. Although these possibilities cannot be discounted, we do not believe that in the profiles examined they account for more than a minor part of the variations observed.

The peat profiles were collected as continuous monoliths (always at least 100 m from any road), divided into appropriate samples in the laboratory (usually 1 cm thick slices), and digested and analysed as for the moss samples. Any large macroscopic remains in the samples were removed by hand before drying. Figure 1 shows the lead content of a peat profile collected within 1 m of an earlier radiocarbon-dated profile from the Snake Pass, Derbyshire<sup>6</sup>, so that there are

unlikely to be any substantial errors in the positions of the datings shown. The site (grid reference SK 091928) is some 9 km north of the nearest Derbyshire lead mining area, but is also more or less directly downwind of the Manchester conurbation. A small peak in lead values in the profile around AD 500 may represent early (Roman) lead mining activities in Derbyshire. A sustained increase in lead values occurs around AD 1460, and reaches peak concentrations of about 600 p.p.m. (as compared with the background concentrations of about 8 p.p.m.) at the same depths in the peat at which soot contamination first becomes obvious. Lead values in the surface 5 cm of peat, however, are some 25% lower than the peak concentrations. Thus at this site there has been very considerable pollution with lead, and the major pollution correlates well with the industrialization of the surrounding area and the rise of the Derbyshire lead industry.



**Fig. 1** Lead analyses of a peat profile from the Snake Pass, Derbyshire. The datings shown are radiocarbon determinations Q-855 and Q-849 to Q-853 (ref. 6).

Figure 2 shows the lead content of the upper layers of a peat profile from Buxton, Derbyshire (grid reference SK 028711), close to the sites from which most of the Derbyshire moss samples were collected. Radiocarbon dates are not available for this profile, and tentative datings have been made instead by pollen analysis. The site is only about 2 km away from the main limestone outcrop of the White Peak, and about 3 km away from Buxton, and historical data relating to land use on the limestone and to the growth of Buxton have been used, therefore, to date certain features of the pollen diagram. Table 2 shows the suggested datings. Although any single dating by itself can only be speculative, the eight datings

taken together form a completely consistent series, which agrees well with the Snake Pass datings. The datings give a rate of peat accumulation varying from 1 cm per 20 yr to 1 cm per 17 yr, and these limits have been used to construct the time scale shown on Fig. 2.

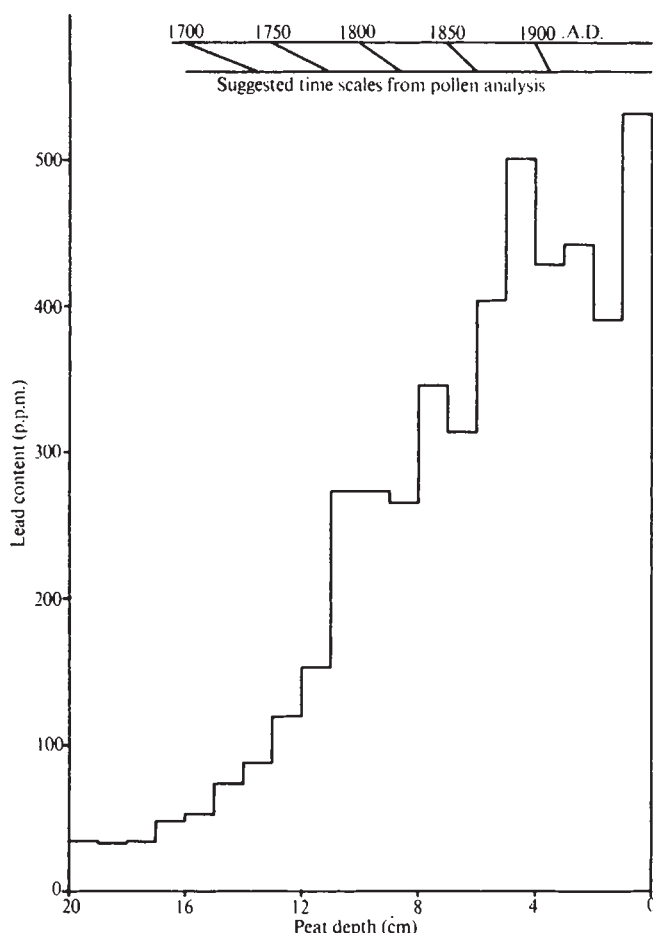


Fig. 2 Lead analyses of a peat profile from near Buxton, Derbyshire.

The Buxton profile shows an increase in lead contents beginning in the late seventeenth or early eighteenth century, and increasing to a maximum in the late nineteenth century. The increase clearly parallels the growth of the lead industry in Derbyshire. The small dips in the late eighteenth and early nineteenth centuries may represent the decreases in lead production due to technical problems and the slump in prices following the Napoleonic Wars. The surface sample has the highest lead content, but this is only slightly higher than the peak nineteenth century values, and there is a close correspondence between the lead values for the profile and those for the moss specimens considered above.

Peat and moss samples from north-west Scotland both indicate low levels of pollution there. The peat profile from Glenshieldaig, Wester Ross (grid reference NG 844484), shows a steady increase in lead content towards the surface (Fig. 3), but there is no evidence of a dramatic increase in the topmost layers. The exact dating of this profile by pollen analysis has not yet been completed, but if a rate of peat accumulation similar to that of the southern Pennines is assumed (that is, about 1 cm per 20 yr), then any clearly observable effect of motor vehicles should be found in the surface 2 cm of peat. The results show that although the lead content of these two samples is a maximum for the profile, there is no indication of a sharp increase from the concentrations of the immediately

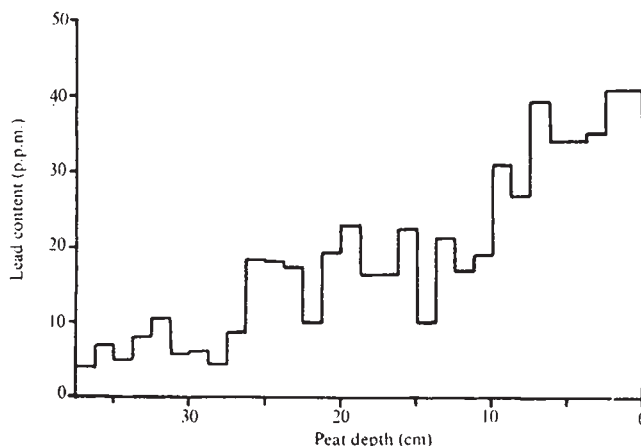


Fig. 3 Lead analyses of a peat profile from Glenshieldaig, Wester Ross.

underlying samples. A similar pattern of lead values has been observed in several peat profiles from northern Scotland. There is no indication of a decrease in lead content in the surface layers of these peats, as might be expected from the moss analyses (Table 1), so that here there is apparently no close correlation between peat and moss samples. The samples from this region of low background pollution show, however, that the exhaust from motor vehicles has had little effect on the lead concentrations in vegetation away from busy roads; and we conclude from the other evidence presented above that the overall regional pattern of lead pollution has been little affected also.

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## The Giant Panda is a Bear

EXOTIC habitat, rarity, striking coloration, and an appealing countenance have combined to surround the giant panda (*Ailuropoda melanoleuca*) with an aura of mystery unmatched by most other mammals. Nonetheless, we really know little about the species, a lack perhaps best exemplified by the long-standing controversy about the giant panda's phylogenetic position among the artoid (canoid plus pinniped) carnivores. There is general agreement that its closest affinities are with the ursids (bears) or procyonids



(raccoons), but then the argument begins. The usual possibilities considered are: (1) The giant panda's closest relative is the lesser panda (*Ailurus fulgens*); as this is usually classified among the procyonids, the giant panda should be also. (2) The two pandas belong together in a separate arctoid family. (3) The giant panda is a somewhat aberrant bear and therefore belongs among the Ursidae<sup>1,2</sup>.

We have previously approached problems of this sort in primates<sup>3-5</sup>, other carnivores<sup>6</sup>, and frogs<sup>7</sup> by immunological studies of albumin evolution. Blood and tissue samples from the giant panda, Chi-Chi, were made available by Drs Brambell and Keymer, subsequent to her death in London in July 1972. The samples were placed in a 2-phenoxyethanol solution<sup>8</sup> and sent to Berkeley by air at ambient temperatures (to preclude refrigeration problems in transit).

**Table 1** Transferrin Immunological Distances among the Ursidae and Procyonidae

| Anti- <i>Ursus americanus</i>               |    | Anti- <i>Procyon lotor</i>             |    |
|---------------------------------------------|----|----------------------------------------|----|
| <i>Ursus americanus</i> (black bear)        | 0  | <i>Procyon lotor</i>                   | 0  |
| <i>Ursus arctos</i> (grizzly bear)          | 6  | <i>Bassariscus astutus</i> (ring-tail) | 16 |
| <i>Helarctos malayanus</i> (sun bear)       | 8  | <i>Nasua nasua</i> (coati)             | 22 |
| <i>Tremarctos ornatus</i> (spectacled bear) | 8  | <i>Bassaricyon gabbii</i> (olingo)     | 25 |
| <i>Thalarcos maritimus</i> (polar bear)     | 11 | <i>Potos flavus</i> (kinkajou)         | 28 |
| <i>Ailuropoda melanoleuca</i> (giant panda) | 18 | <i>Ailuropoda melanoleuca</i>          | 55 |
| <i>Ailurus fulgens</i> (lesser panda)       | 52 | <i>Ursus americanus</i>                | 60 |
| <i>Procyon lotor</i> (raccoon)              | 55 | <i>Ailurus fulgens</i>                 | 64 |

**Table 2** Albumin plus Transferrin Immunological Distances\* among *Ursus*, *Procyon*, *Ailuropoda*, and *Ailurus*

|                   | U  | A   | A   | P | Net change † |
|-------------------|----|-----|-----|---|--------------|
| <i>Ursus</i>      | 0  |     |     |   | 0            |
| <i>Ailuropoda</i> | 22 | 0   |     |   | 0            |
| <i>Ailurus</i>    | 74 | ? ‡ | 0   |   | +6           |
| <i>Procyon</i>    | 85 | 87  | 100 | 0 | +3           |

\* Obtained using antisera to the albumins and transferrins of *Ursus americanus* and *Procyon lotor*.

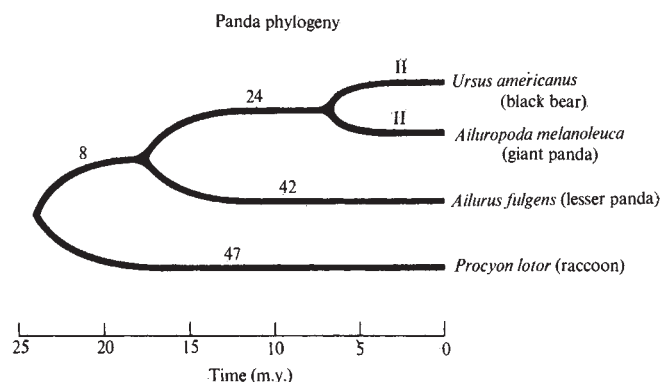
† This figure is the amount of change (AID plus TID) along that lineage relative to *Ursus*. For example, the +6 for *Ailurus* indicates that *Ailurus* albumin plus transferrin have accumulated six more units of change than those of *Ursus*. Data obtained using antisera to *Mephitis*, *Canis*, *Enhydra*, and *Odobenus* albumins and *Canis*, *Mustela*, *Zalophus*, *Phoca*, and *Mirounga* transferrins.

‡ Antisera to the *Ailurus* and *Ailuropoda* proteins have not yet been prepared.

Our previous studies of carnivore albumin evolution<sup>6</sup> had shown that the ursid albumin lineage had accumulated substantially fewer amino acid substitutions than those of other carnivores, thus markedly reducing the resolving power of this molecule in any ursid-related matters. We had already begun a study of the evolution of carnivore and primate transferrins to parallel our albumin work, and preliminary observations gave two indications of the particular use of the transferrins for the panda study. First, we found that transferrin immunological distances<sup>6</sup> (TID) averaged some 60% greater than the corresponding albumin distances<sup>9</sup> (AID); and second, that ursid transferrin evolution was not conservative. For example, the mean *Ursus*-feloid (*Felis*, *Hyaena*, *Arctogalida*) TID of 131 units is very similar to that found for *Canis* (127), *Mustela* (131), or *Mirounga* (135).

The overall carnivore transferrin results and conclusions

are in preparation for publication elsewhere and only those results directly relevant to the panda questions will be mentioned here. The transferrins were purified using the following sequence<sup>10</sup>. Rivanol precipitation of the other serum proteins, treatment of the supernatant with (NH<sub>4</sub>)<sub>2</sub>SO<sub>4</sub> retaining the fraction precipitating between 40 and 70% saturation, and then preparative polyacrylamide gel electrophoresis. The purified transferrins were injected into rabbits (four per species) according to a published schedule<sup>6</sup> using 1.5–2.5 mg of transferrin per rabbit over the course of the immunization period. The resulting antisera were pooled in reciprocal proportion to their microcomplement fixation (MC/F) titres<sup>11</sup> and all quantitative immunological comparisons carried out using the MC/F technique. The differences among the transferrin (and albumin) species tested are expressed in immunological distance units, where one unit is approximately equal to one amino acid substitution<sup>12</sup>.



**Fig. 1** A phylogeny of the two pandas based on the data of Table 2. The numbers along the lineages indicate the amounts of albumin plus transferrin change (measured in immunological distance units) along them. The time scale is applied using a figure (in a conjectural sense) of 250 units of albumin plus transferrin change for 60 m.y. of separation between mammalian lineages, based on our studies of those two proteins among primates, carnivores, and rodents.

In view of the basic question, "Is the giant panda a bear or a raccoon?" it is interesting to note that the closest interfamilial association among the arctoid carnivores (except for the three pinniped families) is between the ursids and procyonids. Even after correcting for the conservatism of ursid albumin evolution, the summed *Ursus* to *Procyon* albumin plus transferrin immunological distance of ninety-seven units is still less than for any other inter-taxon comparison among the taxa Canidae, Ursidae, Procyonidae, Mustelidae, and Pinnipedia (the range among these five taxa is 110–150 units). Thus it would seem that the ursids and procyonids share a common ancestral lineage subsequent to the divergence of those leading to other arctoids—an observation which tempers the bear-panda-raccoon controversy to an appreciable extent and circumscribes effectively the problem of the position of the pandas.

The data specifically relevant to the panda are given in Tables 1 and 2, and their phylogenetic implications in Fig. 1. From the data of Table 1, obtained with antisera to the transferrins of the black bear and raccoon, it is clear that the giant panda belongs to the ursid lineage although the other bears tested share a period of common ancestry subsequent to the divergence of the giant panda lineage. The closeness of the relationship between the giant panda and the other bears is emphasized by the fact that their TID (eighteen units) is within one unit of those separating other quite closely-related species pairs such as domestic cat and lion, dog and fox (*Vulpes*), or baboon (*Papio*

*anubis*) and macaque (*Macaca mulatta*). These data also show that, though the New World procyonids form a fairly compact group, the lesser panda associates neither with them nor with the giant panda. In fact, if the albumin and transferrin data are taken together (Table 2), they tend to place the lesser panda lineage as a very early offshoot from the ursid line, though this latter hypothesis awaits further testing using both molecular and comparative anatomical evidence.

It should be noted that an immunological conclusion of the giant panda's ursid status is not a new one. Some years ago, Leone and Wiens, using antisera to whole sera and the turbidimetric technique, also concluded that the giant panda was a bear<sup>13</sup>. It should be pointed out, however, that questioning their conclusions concerning the giant panda, could raise certain issues and this study was undertaken to try to resolve these. In particular, because whole serum reactions are usually mostly albumin reactions<sup>14</sup>, the bear-giant panda association could have been due simply to conservatism of albumin evolution in both the giant panda and ursine lineages. The use of the transferrins in this study answers this potential criticism of Leone and Wiens's work and at the same time reinforces the validity of the conclusion they reached.

It is of some importance to point out that our panda conclusions accord almost exactly with those drawn by Davis in his anatomical study of the giant panda<sup>1</sup>. To exemplify the general agreement between molecular and more classical phylogenies, I should like to end with a quote from Davis's monograph.

"Every morphological feature examined indicates that the giant panda is nothing more than a highly specialized bear . . . *Ailurus* resembles *Ailuropoda* in several features involving the masticatory apparatus and the fore foot, the two extremely specialized structures in both genera. Some students (e.g. Gregory) associated *Ailuropoda* with *Ailurus*, and therefore with the Procyonidae, on the basis of these resemblances. There is also a resemblance in the male external genitalia, a structure that is curiously aberrant in these two genera. In all other morphological features *Ailurus* obviously has nothing to do with *Ailuropoda* or the Ursinae. The similarities that Gregory and others have emphasized are convergences resulting from similar functional requirements; they are not similarities resulting from common ancestry. The proper allocation of *Ailurus* is somewhat uncertain. Except for the specialized conditions mentioned above, its morphology resembles that of the Procyonidae more closely than that of any other family, but these resemblances may represent common primitive features. I am not sure that *Ailurus* belongs with the Procyonidae, which is where all modern authors place it, but in the absence of clear evidence to the contrary it seems best to leave it there" (p. 322.)

*Ailurus* samples were provided through the Rutgers Serological Museum by Dr Boyden and by Dr Mottram of the San Francisco Zoo. The technical assistance of Anne Hill and National Science Foundation support to the author and to Dr Allan Wilson, as well as the cooperation of Mr Reuther of San Francisco Zoo, and Drs Brambell and Keymer of London Zoo, are acknowledged.

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## Whales' Respiratory Volume as a Possible Resonant Receiver for 20 Hz Signals

SINCE the advent, some 25 yr ago, of recording equipment responsive over a wide frequency band, listening stations at scattered ocean locations have recorded long, repetitious trains of powerful, low frequency sound pulses that vary over a narrow 6 Hz band centred at about 20 Hz<sup>1,2</sup>. Once an enigma, the source of these remarkable signals is now thought to be baleen whales, and although several types of signals have been recorded<sup>3</sup>, implying that more than one species is responsible, strong evidence implicates the finback whale, *Balaenoptera physalus*, as one of the generators<sup>1</sup>. The low frequency acoustic mechanisms of whales are as yet unknown<sup>4</sup> but, based mainly on sound propagation theory, it has been both suggested<sup>2,4</sup> and vigorously argued<sup>5</sup> that the whales use the signals for long-range communication.

While considering cetaceans as acoustic targets, I stumbled on an intriguing theoretical analogy. The target strength of fishes depends on the relationship between swimbladder gas-volume and the frequency of the impinging sound waves. When the gas volume is properly related to frequency, it resonates, strongly reinforcing the scattered sound<sup>6</sup>. To evaluate whales, I followed the same approach, substituting the respiratory air for swimbladder gas-volume. I used the 2,000 l lung volume reported for a 21.6 m finback whale<sup>8</sup>, assumed a spherical gas form, calculated the radius of such a hypothetical bubble, and computed its resonance frequency at an arbitrarily chosen depth according to Andreeva's formula<sup>7</sup>. The result so closely matched the signals known to be produced by finback whales that it prompted me to further considerations.

A possible flaw in the above treatment is in the assumption of a spherical gas form for compatibility with Andreeva's formula, but work with smaller, trained cetaceans has shown that total alveolar and bronchial collapse occurs at about 10 m depth. The air is forced out of the extremities of the respiratory tree and is contained in the resilient, cigar-shaped trachea<sup>8</sup>. The same situation may apply to the larger cetaceans. The shape of the trachea approximates a prolate spheroid the acoustic behaviour of which will vary only  $\pm 10\%$  from that of spheroids of equal volume<sup>9</sup>.

Encouraged by the above information, I then generated a family of resonance curves for surface lung volumes ranging from 1,000 to 3,000 l at depths to 100 m (Fig. 1). This treatment indicates that the resonance frequency of all these gas volumes corresponds to 20 Hz within the pressure range exerted by depths of from 36 to 63 m, and that the entire 6 Hz band (17 to 23 Hz) is covered between depths of 27 to



78 m. Thus, neither the size of the whales nor slight variations in their respiratory volume is crucial to the hypothesis with the given assumptions.

How deep do finback whales normally dive? Valid data on depth of sounding for various whale species are sparse but, some 30 yr ago maximum diving depths of from 85 to 355 m were approximated for five finback whales by using mano-

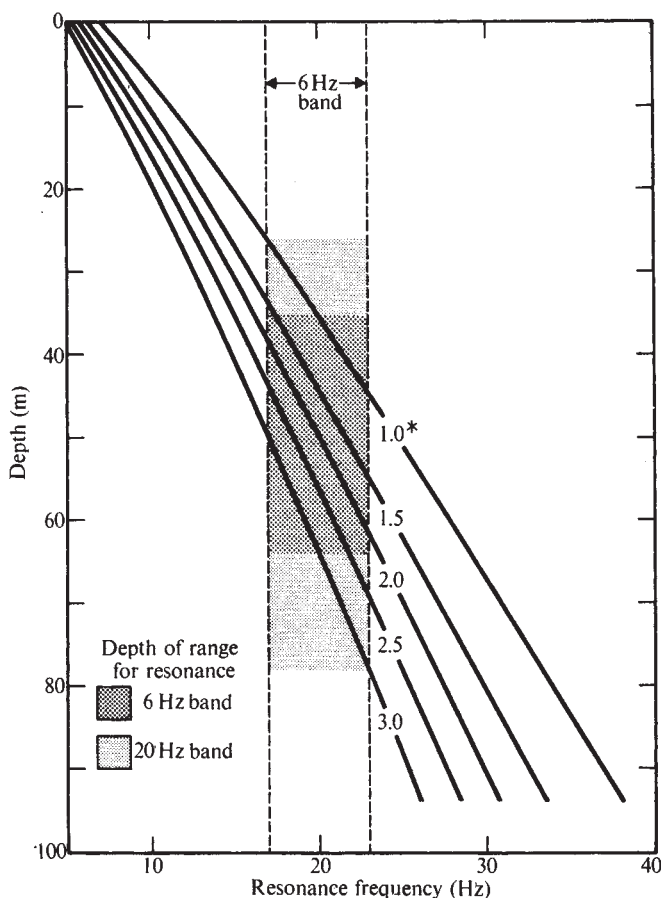


Fig. 1 Resonant frequency curves for finback whale surface respiratory volumes calculated according to Andreeva's formula<sup>7</sup>:

$$f_r = (1/2\pi R) \sqrt{((3\gamma P_0 + 4\mu)/\rho)}$$

where  $f_r$  is the resonant frequency in Hz;  $R$  is radius of a spherical bubble in cm;  $\gamma$  is ratio of specific heats (1.4);  $P_0$  is pressure in dyne  $\text{cm}^{-2} = \text{atm} \times 10^6$ ;  $\mu$  is shear modulus of fish flesh ( $10^6$ ) and  $\rho$  is density of seawater ( $1.02 \text{ g cm}^{-3}$ ). An additional atmosphere for pressure at sea level has been added to the depth pressures. \* Surface respiratory volume ( $\times 10^3 \text{ l}$ ).

meters attached to harpoon lines<sup>6</sup>. Although it is questionable whether harpooned whales behave normally, these data at least indicate that the finback whale is capable of diving to and below those depths that would result in 20 Hz resonance.

The concept of resonance in the auditory sense of cetaceans is not new. From the mid 19th century, it has frequently been proposed that air in the cavities associated with the middle ear responds to sound waves and transmits the amplified signal to the cochlea<sup>10</sup>, and long before mysticete whales were known to produce low-frequency sounds a resonant phonation function was inferred for their laryngeal sacs<sup>11</sup>. But the entire question of sonic and auditory mechanisms, even for the smaller cetaceans that can be subjected to experiment, is still hotly debated. Obviously, the

situation for the larger, unmanageable mysticetes is even more of a mystery.

Ideas on the projecting mechanism of the 20 Hz pulses are contained chiefly in a symposium proceedings<sup>1,12</sup>. An earlier proposed "heartbeat hypothesis" was put down, and the larynx, probably in conjunction with the associated laryngeal sac, was considered the most likely generator. The lungs were suggested as a possible resonant source, but, because the frequency of the pulses had not been shown to vary in accordance with the depth and size of the whales, this idea was set aside. The low-frequency receiving mechanisms of baleen whales were not considered then, however, and, in this context, the possible role of respiratory volume resonance now deserves further examination.

Although peak resonance response to impinging signals would require that a whale dive to and remain at a critical depth, the  $Q$  of the sound spectrum predicts that considerable amplification could still be gained by a whale occupying a selective, but relatively thick, depth stratum. For example, the 6 Hz band indicated in Fig. 1 delineates the half-power down points. Therefore, a 2,000 l whale could occupy some 26 m of the water column (and a 3,000 l whale about twice that) and still benefit from strong resonant enhancement of the signal. Furthermore, if a whale dives below its potentially resonant depth strata, it will pass through this zone again while surfacing. In effect, the model indicates that a whale with a gas-filled resonant structure can both "tune" itself to certain frequency bands and control the "gain" by either regulating the volume of its inspired air or operating within prescribed, but relatively broad, depth limits. The advantages of such a system need no explanation.

Neither the content of the message, if in fact 20 Hz signals are communicative, nor the maximum ranges attainable by the whale's system<sup>5</sup> need be of concern here. Nor is it now appropriate to ponder directional reception or the possible role of the laryngeal sac. It should be mentioned, however, that a resonant receiver that could not also be coupled to the driver would be a poor design.

The case for resonance reception is based solely on what may be a provocative, spurious correlation. But large whales are difficult experimental subjects, and cetology is fraught with conjecture stimulated by anatomical hints and chance observations. Further speculation should not seriously endanger progress.

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# BOOK REVIEWS

## Swansong of a Polymath

*Memories II.* By Julian Huxley. Pp. 269+25 photographs. (George Allen and Unwin: London, 1973.) £4.95.

HAVING first disclosed my interest as an old friend of the author, I must, as the reviewer of this swansong to a distinguished career, consider the career as well as its hero's account of it, and the man himself. Few subjects could be more baffling. Are we concerned with the life of an eminent scientist, an inveterate birdwatcher, a social reformer, a cultural philosopher, a pioneer humanist, a star performer on the platform or at the microphone, an insatiable devourer of knowledge or, as he confesses, a voracious traveller? Where in the mutually reflecting galaxy of Huxleys does T.H. end and Julian begin, or Julian lead and Aldous follow without knowing where to halt? How does one disentangle the cosmic, timeless, far-ranging aspect of his imagination and studies from the restless, ephemeral, energy-dissipating motion of a character who sometimes makes one feel that mass communications and the jet age were a response, however coincidental, to the pattern of living which his dynamic called for? What has been his influence, and where has it been most felt? Is he, as he sometimes implies, a prophet ahead of his time—a proposition for which there is plenty of evidence? Or is he rather the natural spokesman of a complex protean contemporary western culture, hybrid between the classical and the scientific, ceaselessly glissading across the Snow line without being able to find a settled abode? Has he made the right enemies for valid reasons or has he to some extent been his own worst enemy?

If, perish the thought, there were a regular profession of autobiography, Julian Huxley would rank as one of its accomplished performers. He writes, at 86, in a crisp untiring style, giving life and pace to his tale, and even taking two volumes he has so much to tell that he is never long-winded. He achieves an engaging blend between profundity and triviality, and between the public record and the personal background. He is often open and revealing about his own doubts and weaknesses,

and considering the intense and not always fair opposition which he has encountered he is remarkably charitable and free of self-pity, giving ample credit to others.

If faults are to be found it may be thought by some that the travelogue and diary of events tend to overshadow the rest, and that the explanation and drama of his many crusading campaigns are cut down to an incidental and superficial element. Critics who compare facts and even dates with such other sources as Ronald W. Clark's *The Huxleys* will find a number of discrepancies and confusions in the detailed record. The proportionate space devoted to different events and episodes sometimes seems to reflect the relative ease of retelling them rather than their significance.

This second volume starts appropriately with the odd and unexpected appointment of the author as first Director-General of UNESCO, of which he remarks, "I was flabbergasted. . . . I knew nothing about the organizational aspects of such a body, and felt I would be at sea in such a post, but promised to think it over. . . . Though I felt like one of those early Christians who were kidnapped and forced to become bishops, I announced my agreement . . . and went home to report to Juliette. She was shocked. . . ." Whatever some may feel with hindsight over the validity of his doubts, his acceptance was an important step in the history of science. The original scheme had been for a UNECO, devoted to education and culture but excluding science from them. It was Julian Huxley's eleventh-hour intervention which had remedied that blunder already, but without his leading role in launching the new agency there is little doubt that science would in effect have been shouldered out, and under some of his successors it has sometimes seemed that the name was assumed to be UNEsCO, with a very small "s". His early difficulties were aggravated by some unwise appointments, strongly biased in favour of leftist intellectuals, and it may well have been these, rather than his personal opinions, which led to his being accused,

as he records, and by Lady Zimmern in particular, of being a communist. Considering the barely workable constitution, the lack of precedent and organized support, the national jealousies and constraints and the staggering diversity of the agenda it was a remarkable achievement to get the agency firmly established, all the more so because objections led to his being allotted only a two-year term of office instead of the standard five.

While probably no later chapter is so important as a first-hand source, scientific historians will find much valuable material about the origins and growth of many subsequent initiatives which have helped to set trends and to shape organizations, in many cases with the early participation of the indefatigable Julian Huxley. His final chapter is entitled "Ageing, but still active", and it is most fortunate that one of these latest fruits of his far from negligible continuing activity has been the completion of this valuable and well written account of a life spent in studies and action highly relevant to the problems of today and also of tomorrow.

E. M. NICHOLSON

## Essential Physics

*Mechanics and Electrodynamics.* By L. D. Landau and E. M. Lifshitz. Pp. xiii+282. (Pergamon: Oxford and New York, May 1972.) £3.

IN the years before his tragic death Landau had the idea of producing a course of theoretical physics giving the "minimum of material" with which every physicist should nowadays be familiar, but containing much less than the volumes of the well known *Course*. The present volume is essentially an abridgment of the *Mechanics* and *Classical Theory of Fields* volumes of the larger *Course*. The translators are J. B. Sykes and M. Hamermesh.

The first impression given is one of surprise that so much that is useful and relevant can be included in a rather small book. Problems with solutions are provided and there is at the beginning a useful summary of the notation.

In part I, the mechanics section begins with Hamilton's principle (here called the principle of least action) and gives a concise and elegant account of the relation between invariance and conservation laws. Central orbits, collisions, oscillations, motion of a rigid body, and non-inertial frames of reference are discussed. Hamiltonian mechanics, the Hamilton-Jacobi equation and adiabatic invariants conclude this section.

(Any physicist capable of reading the book will not be disturbed by equation (19.6) where a coordinate of an oscillating system is represented by a complex number.) Two chapters on the special theory of relativity follow.

Part II deals with electrodynamics and begins with an action principle, the Hamiltonian for a charged particle in an electromagnetic field, the electromagnetic field tensor. Thereafter a surprising number of useful topics are discussed in an elegant and economical manner. These include the electromagnetic moments of systems of charges, electromagnetic waves, diffraction, retarded potentials, radiation damping, and scattering.

There are a very few things which I should like to see changed; for example, it is not shown that the retarded potentials satisfy the Lorentz condition which was assumed in their derivation. However, these are small points in comparison with the scope and economy of the treatment. The physicist who is thoroughly versed in this "minimum of material" will be well equipped. It may be that to learn the material from this book he will have to read carefully and reflect. However, he will probably enjoy doing it and it will be a good training for the persevering student.

CHARLES STRACHAN

## People Need Protein

*Proteins in Human Nutrition.* Edited by J. W. G. Porter and B. A. Rolls. Pp. xi+560. (Based on the Proceedings of a NATO Advanced Study Institute on the Chemistry, Biology and Physics of Protein Evaluation, held in Reading, England, March 1972.) (Academic Press, London and New York, 1973.) £9.50.

A GREAT deal has been published in the last few years on the subject of proteins in human nutrition, and my first reaction to the title of this book was that it was unlikely to contain much that is interesting or new. I soon found this impression to be entirely wrong: the book is a most valuable contribution, informative, lively and stimulating. It is arranged in four sections: protein supplies and requirements; the evaluation of protein quality; the role of food science and technology; and factors affecting the utilization of proteins.

Three main themes can be identified in this symposium. The first is the problem of applying existing knowledge, particularly in relation to the needs of developing countries. Mauron, in his introduction, throws out the challenge: "We are now aware that protein malnutrition is more an economic problem than a medical one, more a matter of food distribution than of production. . . . In few fields is there such a distressing gap between knowledge and its implementation".

The opening chapters on world supplies of protein paint on the whole, rather surprisingly, an encouraging picture. Jalil and Tahir from FAO conclude that "there is a solid basis for optimism that rapid progress can now be made in closing the protein gap", and Worgan, writing on unconventional sources of protein, states that in relation to total food output there need never be a deficiency of protein. To produce food, however, achieves nothing unless it can be purchased and eaten. Other contributors from FAO make the point very clearly that dietary adequacy is directly tied to income, and emphasize the importance of distribution of income and purchasing power. This is brought up again by Rosen, in an article on the use of oilseed proteins, who stresses that "it is only by greater study of and greater emphasis on these less scientific (that is, economic) factors governing the use of proteins that we shall start to be able to stem the increasing gap".

The second theme is whether there really is a protein gap, or whether the real problem is an energy gap or a total food gap. There is a clear difference of opinion among the contributors, although it is not argued out in any detail. Payne states that "At the physiological level probably no country has an overall shortage of protein, so that the problem, if any, is one of unequal distribution . . .". The phrase "at the physiological level" is a reminder that the question whether there is a protein gap can only be answered if we know the requirements for protein. The one deficiency in this book is that it contains very little discussion on this subject. A remark quoted from Cathcart is pertinent: "Any diet can be regarded as defective if you put your so-called standard high enough". Whitehead's chapter on the protein needs of malnourished children provides practical confirmation of the rather low estimates of protein requirements put forward by FAO/WHO; these form the basis for Payne's point of view.

The third theme, which takes up about half the book, is that of protein quality and protein value. It is fascinating to find that after more than sixty years of intensive work on this subject, there was almost total disagreement among the

participants on nearly every aspect of it. Hegsted emphasizes the discrepancies between chemical score and biological assays. In a trenchantly critical chapter he concludes that there is no such thing as a reliable bio-assay of protein quality. Other contributors take a much less pessimistic, or perhaps less critical, attitude. Equally, there is disagreement about whether results obtained in rats can be applied to man; whether the same measurements of protein quality apply for maintenance and for growth; and even whether estimates of protein quality have any relevance to actual human diets. Pirie has the last word: for thirty years he has claimed that a numerical measure of protein value is impossible, but no one has paid attention.

The later chapters are less controversial; they deal with the problems of the food technologist in assessing and predicting qualities such as texture and flavour, and with factors which impair the utilization of proteins. Liener's chapter on toxic factors in protein foods is outstanding, and makes one wonder whether it is safe to eat.

A multi-author book of this kind does not come together by itself. The editors are to be congratulated on the way in which the balance and standards have been maintained. The printing and layout are also exceptionally good. The book as a whole will be of value not only to the worker who already has some experience, but also to the more general reader who wants an over-view of current research and thinking in this field.

J. C. WATERLOW

## Organisms under Pressure

*The Effects of Pressure on Organisms.* By M. A. Sleight and A. G. MacDonald. Pp. xii+516. (Cambridge University: London, September 1972.) £8.

THIS book should prove to be a welcome addition to the literature dealing with the influence of pressure on biological processes, providing, as it does, a convenient, authoritative, and broadly comprehensive source of information to anyone who is carrying on research in this field or is potentially interested in taking it up. Topics ranging from basic aspects of the physics and physical chemistry of increased pressures to the effects of pressure on relatively simple molecular systems as well as on such complex processes as adaptation, vertical migratory behaviour of fishes, and physiological processes of both marine and terrestrial animals including man, are included among the twenty-four articles plus three abstracts based on papers given at the 26th symposium of the Society for Experimental Biology, 1971. The symposium organizers deserve congratulations for, among other accomplishments,



bringing together from many parts of the world the most outstanding investigators currently active in research on various aspects pertinent to the general subject.

At the beginning, the editors make some apposite remarks, for example, to the effect that although the symposium papers make it evident that pressure is as important an influence as temperature on the lives of many (especially marine) aquatic organisms, the literature on pressure is much less than that on temperature. This observation is underscored by the fact that, within the last almost 100 years, this is only the third book to be published dealing primarily with the biological effects of increased pressure: the first was by P. Regnard in 1891 and the second, edited by A. M. Zimmerman, appeared as recently as 1970.

In their introductory remarks the editors seem to take too restricted a view in the statement that "The fundamental effect of pressure is to favour changes that involve a reduction in volume; . . .", whereas it is certainly true that a no less fundamental effect of pressure is to oppose changes that involve an increase in volume, inasmuch as both sorts of changes may assume critical importance in reactions involving large molecules such as the enzymes or other proteins and the nucleic acids characteristic of biological systems. Further, equally fundamental aspects of the effects of pressure are that the net result, in regard to the magnitude and the direction, that is, inhibition or stimulation, can be determined (1) by temperature, and (2) at a given temperature, by the chemical environment, for example, as in the reversal of narcosis by increased hydrostatic pressure. Although such relationships were revealed and interpreted with some success with the aid of the theory of absolute reaction rates more than a generation ago, they are not taken into account in various instances wherein they might seem useful in arriving at a clearer understanding of the phenomena involved, and, for the most part, they are only vaguely or casually mentioned in the symposium papers.

The broadly inclusive and indeed highly informative treatment of the subject conveys something of an impression that the observed results of experiments are generally due to pressure *per se*, whereas different conditions and different ranges of pressure require different avenues of theoretical interpretation. For example, increased pressures will increase or decrease the rate of a chemical reaction in homogeneous solution in accordance with the amount of pressure and the molecular volume change involved in formation of the activated (transition) states from the reactants. A volume change

of this sort is never great enough to be measurable by comparing reaction rates at atmospheric and below-atmospheric pressure. When a gas phase is present, however, as in air-breathing animals, changes in pressure in either direction, that is, to greater or less than one atmosphere, are accompanied by changes in concentration of reactants, thereby introducing added complications, for example, in the physiology of diving mammals and of man in space. Additional complications arise in a deformation type of pressure rather than a uniformly acting, hydrostatic type.

Manifold opportunities for fruitful research are evident in this book, notably studies on animals indigenous to the deep sea, a type of study which Regnard did not significantly achieve and which is only now being seriously undertaken by some of the authors represented in the book.

For a number of reasons, this symposium volume may be considered indispensable to anyone interested in the general subject. FRANK H. JOHNSON.

## Physics in Archaeology

*Methods of Physical Examination in Archaeology.* By M. S. Tite. Pp. xxx+389. (Seminar: New York and London, February 1973.) £6.80.

In recent years the application of physical methods to archaeology has grown in importance. It is true that numerous fascinating methods have long been available, but because of lack of resources or entrenched archaeological attitudes, they have been comparatively little used. The situation is now changing, however; the scientist's enthusiasm for devising ever more refined methods is being matched by their application to archaeological problems, and the tendency to appoint scientists to the staffs of university archaeology departments in helping to promote the integration of these techniques as part of the accepted methodology in the study of the past. The time is now ripe for a handbook on the subject and this is admirably provided by Dr. Tite's *Methods of Physical Examination*.

The book covers a broad spectrum: techniques for locating archaeological sites, age determination and methods for analysing the composition of artefacts. Within each of these fields, Dr Tite carefully and lucidly describes nearly all the methods of proven value, including very recent developments, and in each case he gives useful examples showing how the techniques have been applied in archaeology. Most of the information has been culled from the existing literature and there is little that is new, but this is no criticism, for it is most useful to have this body of information under the same cover for the first time. If the

archaeologist wishes to know what work has been done on, for example, the characterization of a certain type of artefact or if he wishes to discover what method would best solve a particular problem, Dr Tite's book will provide a ready and authoritative answer.

The work has been carefully prepared and thus there are few points to criticize, though I could not help noting that one of my suggestions had been inadvertently attributed to another worker! The choice of plates is, however, somewhat strange, for many show little of interest or serve no useful purpose. For example, Plate 1, of the Overton Down experimental earthwork, receives only passing mention in the introduction; Plate 7 of excavated pits and ditches at South Cadbury shows nothing that is not better illustrated in Fig. 18; while no less than two plates are devoted to the weathering laminae in ancient glass, which, as Dr Tite himself remarks, are of dubious value in dating. One has the impression that the author was trying to meet his publisher's demand for twenty-five plates, but with a little imagination these could have been much more instructive.

These criticisms are dwarfed, however, by the overall value of the book. It should certainly have a place on the shelves of all archaeological scientists (whether they work in science or in archaeology departments) and it will also be very useful to many archaeologists. The book will serve as a much-needed undergraduate text for students with some scientific grounding, but the arts based man will find the scientific jargon and formulae rather formidable.

D. P. S. PEACOCK

## Invertebrate Vision

*Information Processing in the Visual Systems of Arthropods.* Edited by R. Wehner. Pp. xi+334. (Springer: Berlin and New York, 1973.) 36 DM; \$10.50.

THIS book contains the papers given at a conference held in March 1972 on the arthropod visual system. The participants were mostly from Germany and Switzerland, and a few were from Britain. The contributions are, however, all in English which, it seems churlish to say, is not always easy to understand. The papers covered a wide range of topics, including the anatomy of the optic lobe, the optics of the compound eye, pigment biochemistry, colour vision, pattern recognition, and orientation behaviour.

The stated intention of the book is to provide a summary of recent work on the arthropod visual system for people in a variety of fields, and particularly



for those in vertebrate neurophysiology. Unfortunately, this aim was not achieved. Most of the chapters are short (average length seven pages) reports of current research on insect vision. Because of their brevity they make few concessions to those unacquainted with the history or significance of the problems. There are, however, some notable exceptions. For example, the paper by K. Kirschfeld is an up-to-date account of his elegant experiments on the fly "neural superposition eye", and M. F. Land has provided a lucid review of his work on the orientation and pattern recognition behaviour of jumping spiders. Although the book fails in its broader purpose, it is for the specialist a useful and interesting collection of research papers.

One of the hopes of the editor is that this book will display the advantages of the arthropod visual system for relating visual physiology and behaviour. But the impression gained from this collection is that, with the possible exception of the optomotor system, there is still a very wide gap between physiology and behaviour. Our knowledge of the physiological organization of the optic lobe is restricted to the periphery; beyond the photoreceptors and second-order cells it is very sketchy. Apart from the papers by R. Hengstenberg, W. Kaiser, and F. Zettler and M. Jaervilehto all the neurophysiological contributions in this book deal with receptors or electroretinograms. On the other hand, the ingenious and elegant behavioural mechanisms adopted by arthropods are amenable to sophisticated behavioural analysis, as shown beautifully by the papers of M. L. Mittelstaedt-Burger and P. Goerner on course-correction and of R. Wehner's group on the various cues ants use to guide their way home. But we are still very far from understanding the physiology of behavioural mechanisms of this complexity.

THOMAS COLLETT

## Beyond Reason

*Science and Christianity: A Partnership.* By Robert E. D. Clark. Pp. 192. (Pacific Press: Mountain View, California, 1973.)

ABOVE the blackboards in the main physics lecture theatre of a Scottish university where I once worked there used to be written in large letters: "Truth will in the end always flow in the direction of the greatest speculative reflection." I never fully understood the significance of this text; but it could equally well do as a short summary of this book by R. E. D. Clark. Dr Clark is a professional organic chemist, and he has written this book to show not so much that there is no conflict between science and Christi-

anity, but rather that the parallels between them are so close that they may be called a partnership. Precisely this word is the sub-title of the book.

Dr Clark sets out to show how similar are the processes at work in a scientist and a Christian. The greatest of these is creativity, for he sees the innovation of the scientist, and of the religious person, as a kind of quantum jump—a creative act that transcends reason. "Logic", he writes, "is the enemy of discovery". To discover, we must be prepared to abandon the past, to believe at least temporarily in things which seem absurd, to accept "reason's humble role", and, having once started on the high road to discovery, not to stop till more and more has been accomplished—certainly not spend time arguing about it with your friends, for then the new light will flicker and fade away.

It is not surprising that such a view involves the author in a strong anti-authoritarian attitude: for the pundits never accept new truth (here there is an echo of Max Planck, who argued that new scientific ideas were not accepted until the older generation had actually died). Our author evidently dislikes the establishment, whether in Church or science, as much as any of those younger folk who are only half his age. Time after time he draws attention to errors accepted by scientific leaders; and he buttresses his argument with a superb collection of quotations and illustrations, many of which were completely new to me.

But is it all as simple as this? Surely the author overstates his case when disparaging the reaction of the scientific establishment to new ideas. For example, although a full acceptance of relativity did take about 30 years, Schrödinger's wave equation of 1926 was universally established and taught by 1936, and the Watson-Crick model of DNA took no longer. The establishment may be slower than some would wish; but the pace of modern scientific discovery, and the number of good scientists all over the world, soon leads to the clearing-up of residual doubt.

Nor is it quite true to argue that you have to forget the past if you would discover the future: it is the "prepared mind" which is most often visited by chance.

Accepting this we come to the sub-title of the book. If science and Christianity are partners, then what are they partners for? And do they ever disagree as they go about their joint responsibilities? The answer to the second question is, surely, yes. There do remain disagreements which no doubt the passage of time will help us to remove. But he would be foolish who pretended that they did not exist. As for the first question, this is left unanswered. If the answer is that both

are seeking truth, we compel ourselves to enquire just what are scientific truth and religious truth. The methodologies of the two movements may be closely parallel, as Dr Clark shows; but there is more both in science and in Christianity than methodology. The author has written an interesting book, but there is still need for a sequel, to lead the reader from methodology to content.

C. A. COULSON

## Inconstant Stars

*Variable Stars.* By W. Strohmeier. Edited by A. J. Meadows. Pp. viii+279. (Pergamon: Oxford and New York, December 1972.) £6.50.

VARIABLE star research is one of the most active fields of astronomy. Observations and theories of stellar pulsation are fundamental to our knowledge of stellar structure. Much work on little understood phases of stellar evolution centres on variable star studies and these objects retain their importance for studies in galactic structure. The whole field has been given enormous impetus by the application of new techniques which have revealed many unexpected phenomena. Infrared and other work has shown the importance of dust shells around some variables. Some red variables show emission at microwave frequencies. X-ray stars are often variable at either X-ray or optical wavelengths. Recent years have seen the study of objects (for example, eruptive binaries and, of course, pulsars) which vary on a very short time scale.

It would be very difficult for one person to do justice to all aspects of this large field at research level. A general account at a slightly lower level is desirable, however. Professor Strohmeier is an active variable star worker and his book will no doubt be useful to many readers. But mainly because of his attempt to get everything in (quasars in two pages), the book tends to become a rapid summary of facts and conclusions and some theory with little indication of how conclusions follow logically from facts. As a result, the book may prove useful not so much as a means of understanding the subject but as a condensed source of reference. The level of presentation is somewhat variable; for example, there seems little point in giving a description, however brief, of the principle of the photo cell if the UBV system is introduced without explanation. There are some puzzles in the book (Tables 3, 4 and 5 are not referred to in the text, some axes are unlabelled). Table 2 contains some misprints, but is surely memorable for the introduction of a new class of variable. Variables are, apparently, pulsating, eruptive, symbiotic, eclipsing or twitchety.

M. W. FEAST

# CORRESPONDENCE

## Model of Destruction

SIR,—I agree with the conclusions put forward by Joseph Salerno<sup>1</sup> in his article on Forrester's world dynamics model, in which he showed that this model was exceedingly sensitive to the input assumptions. Forrester did not, of course, claim that his model was accurate in detail, although he certainly gave the impression in his book that he believed the fundamental finding—that any model would lead to a destructive situation in a finite time.

I believe that there is a much more serious fault in his model as applied to sophisticated populations. He makes the specific statement (page 18 of *World Dynamics*) "A rate of flow is controlled only by one or more of the system levels and not by other rates". This assumption would be perfectly correct if applied to the interactions of assemblages of molecules, or even of ants or rodents. It is, however, quite untenable when applied to the present world population.

As an example, I would point out that the very considerable fuss, which led to the drastic restriction of the use of DDT in the United States and elsewhere, was due hardly at all to the effects of the actual level of DDT in the environment, but almost entirely to concern about extrapolations of its future effect. These extrapolations

obviously depended upon the rate of increase of use of DDT and not merely on its current level, which it is difficult to show to have been responsible for any deaths among the general public, other than those in handling accidents common to, but less than, other pesticides which have not been banned.

Similarly, the rise in exceedingly sensible concern about world population increase, and such actions as liberalization of the laws concerning contraceptives, and so on, are also based on concern about the rates of rise, and extrapolations therefrom, much more than on the absolute levels. In fact, at long last we are crying before we are hurt, or at least before we are hurt much, which is the sensible and effective time to cry.

If one includes in the computer analysis terms which depend upon rates of increase of the factors involved, there seems no reason why a stable model should not be achieved. Whether such a stable state can be attained in practice depends, of course, on whether we act on our concerns rather than whether we express them. I am not prophesying that we shall achieve a viable stable state and I think that Forrester's book can be regarded as a very valuable warning that we must act on his extrapolations if

we are to have any hope of avoiding them.

Yours faithfully,

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<sup>1</sup> Salerno, J., *Nature*, **244**, 488 (1973).

## Errata

IN the article "Colour Opponent Neurones in the Human Visual System" by Josef P. J. Rauschecker, Fergus W. Campbell and Janette Atkinson (*Nature*, **245**, 43; 1973), line 1, paragraph 7, should read "Monocular rivalry thus depends not only on the relative . . ." not "Binocular rivalry . . .".

IN the article "First Record of the Palaeocene Primate *Chiromyoides* from North America" by Philip Gingerich (*Nature*, **244**, 517; 1973) two lines were omitted from the last sentence of paragraph 9 which should read "The living *Tarsius* has primitive lower molars of the *Pelycodus-Teilhardina-Tetonoides* pattern, but the postprotocingulum is not retained on the upper molars. The remaining living primates have molars derived from the *Pelycodus-Teilhardina-Tetonoides* pattern, the most primitive modification being the development of a hypocone from the primitive postprotocingulum."

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